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THE TIME-SCALE OF THE UNIVERSE[1]

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IT is beyond our power—and may be beyond the bounds of reason—to specify the age of the material universe precisely, like an old epitaph: "Aged 82 years, 6 months and 17 days." But human life has not only an individual duration, but a general time-scale. Only under exceptional circumstances does a man change greatly in thirty days—and rarely does he fail to change greatly in thirty years. There are, therefore, more than astronomical reasons for measuring our ages in years—they afford a scale commensurate with the changes which accompany them.

It is certainly legitimate to inquire, Can we set up a similarly rational time-scale for the description of natural phenomena in the large? Will the units of this scale be thousands of years, or millions, or billions, or greater? and will different scale-units be appropriate for different ranges of phenomena—just as the rapid development of a child in his first year makes it reasonable to give his age in months?

In my student days, at the turn of the century, no definite answer could be given to these questions. We had very strong reasons indeed to believe that millions, rather than thousands, of years formed an appropriate scale for the greater geological processes; but there was no trustworthy evidence concerning how many millions of years were involved.

We realized, too, that, unless wholly unknown forces or influences of some sort were imperceptibly at work, the history of a finite material universe must run its course in a finite interval of time. This conclusion

[1] The ninth James Arthur Lecture, delivered at New York University, May 14, 1940.

was based upon the one physical principle which has withstood unchallenged the revolutionary changes of the subsequent forty years—the Second Law of Thermodynamics. The energy in such a universe is indestructible—though, as we now know, capable of reversible transformation into mass. But it is steadily becoming less available. Water runs, and stones roll, down-hill, and this tends, in the long run, to flatten out the high spots and fill up the low ones. Heat is carried by conduction or radiation from hotter bodies to colder ones. In a thousand other ways, also, nature tends toward mediocrity—a dead level of uniformity and quiescence. There may be other forces—such as the deep-seated ones which build mountains—which, for a time, apparently arrest or reverse the downward tendency, but these draw on other stores of energy—which must be finite in amount—and diminish their availability, so that at long last mediocrity reigns.

This conclusion is unpleasant to most people; but it is supported by the tremendous weight of the Law of Averages. The argument is really simple enough. Suppose we had 10,000 smooth, spherical grains of white sand—all indistinguishably alike in size, shape and weight—and 10,000 more, exactly like the others except that they were black. Pour the white sand carefully, then the black sand cautiously on top of it, into a glass globe of such a size that it is still only half full. Then shake the globe. After a good shaking we will get a grayish mixture. Keep on shaking. What is the chance that by mere luck the white sand will some time all get on the right hand side of the globe and the black on the left? No elaborate analysis is needed to give the answer: it just naturally won't happen. Of course, if the white grains were lighter than the black, shaking would bring them to the top like cream; but we have expressly assumed that this is not so: the two kinds of grains are indistinguishable except by color, and the color makes no difference to the effects of shaking.

The interesting configurations of the material universe, with mountains and valleys, hot and cold bodies, etc., turn out, upon further analysis, to be comparable to interesting configurations of the sand—say a sand-painting on the gray surface. The fault of the analogy is that "shaking" is a catastrophic process, while conduction, radiation, etc., act gradually. But the conclusion that the interesting configurations tend to run down to undistinguished mediocrity is true for both. Technically, this is called an increase of entropy. We shall not be far wrong if we think of it as an increase of mediocrity or of dullness.[2]

Two conclusions follow. First, as regards the future, the universe bids fair, by an overwhelming probability, to become gradually less and less interesting. The approach to the final state of deadly dullness is likely to be asymptotically slow, but sure.

Second, while we can reason thus about the remote future (on the assumption that known natural laws are alone operative) we can not do the same about the past. A homely illustration of Lord Kelvin's comes to mind. Suppose that, in an open field, I pick up a stone so hot that I drop it instantly. I can figure out how long it will take to cool; but all I can say with certainty about the past is that, not so long ago, it has been in a hot place. I can calculate that if it has been lying in the field an hour it had then a certain surface temperature, if two hours, a much higher temperature, and so on, but I can not find out how or when it was heated without further information.

The stars, unlike the stone, are what an ingenuous student of mine once, on an examination, called "self-heating bodies"; but, with suitable changes, the analogy is still valid. We can carry our analysis of the past—a sort of reversed prediction—back a long way, but not forever. Sooner or later our calculations, based on the assumption that the ordinary, vastly probable, processes of "running down" have been continually at work, reach a limit, and we can go no farther back.

It would be unsound to conclude that there must have been some specific intervention of other influences or forces.

In any statistical system—like the globe full of sand-grains—any possible configuration must occur, again and again, in a sufficiently great number of trials, provided the effects of shaking (or its equivalent) are really at random. The universe is at present in a very interesting and improbable state, but this does not *prove* that it may not have got into this state by mere accident.

In a *very* long time, such as $10^{10^{100}}$ years, almost anything may happen. This argument has been clearly stated by J. B. S. Haldane, who caps it with the epigram: "The correct deduction from the Second Law is not that the present state of the universe could not have happened by chance, but that most of eternity is dull."

This we knew, in a general way, forty years ago; but the evidence that the material universe is in a transitory state is far more impressive now than it was then. From the nuclei of atoms to the remotest galaxies, we find irreversible changes—a one-way progress. In several cases we can measure or estimate

[2] I have elaborated this illustration to bring out the importance of the *intrinsically indistinguishable* character of the individual grains—an essential element which has been inadequately stressed in some discussions of the subject. If each grain were marked with a number, and we recorded the position of every one in the gray mixture, and then shook it vigorously, the probability that grain 8392, and every other grain, would come back to the same position it had before would be even less than the chance that all the white grains would be found on one side.

the time-rates of these changes; and one of the most remarkable results of modern physics is that these independent paths of approach lead to the same result. The time-scale, from atoms to nebulae, is measured in billions of years[3]—millions are far too small, trillions much too great. Some of this evidence I will try to summarize, necessarily in brief, without entering upon many fascinating by-ways.

RADIOACTIVITY AND THE AGE OF THE EARTH

(1) Among the major modern discoveries is that atoms of many sorts are unstable and subject to spontaneous disintegration. The atomic nucleus (which automatically clothes itself with electrons enough to build up the rest of the structure) may eject a portion of itself, and settle down into one of a new kind, differing in charge, in mass, or in both. More than twenty such unstable atoms occur naturally, and hundreds of others have been made artificially by bombardment with particles of high energy. With the latter we shall deal later; at present we have to do with the former. These fall into three series commonly called after their most prominent members, radium, actinium and thorium. Each series starts with an almost stable atom, so that only a very small fraction of those present disintegrate in a year. By the delicate and precise technique of radioactive measurements—of which there is no time to speak here—these fractions have been determined with considerable accuracy. Heavy uranium (the abundant isotope U_{238}, of atomic weight 238) loses by spontaneous change, during one year, one out of 6,570 millions of the atoms originally present; light uranium, or actino-uranium, U_{235}, one out of 1,030 millions, and thorium, Th_{232}, one out of 20 billions.[4] The transformed atoms are far less stable, and go in each case through a long series of changes, in some of which alpha-particles (helium nuclei) are ejected, and in others electrons. At last each series ends in a stable nucleus—all three of them isotopes of lead, of weights 206, 207 and 208, respectively. Some of the intermediate products have lives of about a million years; others last only a minute fraction of a second. In all cases, the rates of radioactive change can not be altered perceptibly by any external influence that has been brought to bear. High temperatures and pressures are utterly without effect; and even the drastic process of bombardment with high-speed particles—though it may transform the nuclei which are directly hit into something quite different—has no effect on those which are missed.

On the time-scale with which we have to deal these changes may all be regarded as very rapid, and we may forget the intermediate stages, and talk as if each parent element turned directly into lead, without introducing error into our calculations.

If, now, a uranium-containing mineral is left alone in the rock for a billion years, about 15 per cent. of the atoms originally present will have broken down—to be more precise, 14 per cent., since toward the end there are fewer left to disintegrate than at first. Corresponding to these, an equal number of atoms of lead will have been produced. Their weight will be 12 per cent. of that of the uranium originally present, or 14 per cent. of those remaining—the other 2 per cent. representing helium given off during the process. The older the mineral, the more lead there will be, and from its ratio to the uranium we can find how long the crystal has been undisturbed.

Certain obvious corrections are necessary. If thorium is also present, we must allow for the lead which it has produced at the same time. Actino-uranium is always present to the extent of 1/140 of the heavy uranium, and may easily be allowed for. We must be sure that the crystal has not been altered chemically—for example, by the action of percolating water—during its long entombment in the rock. Hence, only the inner portions of clean crystals, showing no visible effects of this sort, are employed. Finally, how can we be sure that there was not some lead in the crystal when it was formed?

Nature is kind to us here. We can measure, with the mass-spectrograph, the relative amounts of lead of atomic weight 206, 207 and 208. "Ordinary" lead contains also a small proportion of another isotope, of weight 204, which does not appear to be produced by radioactive processes. Measuring this, we can determine and allow for the ordinary lead which may have been present at the start.

With these precautions, the radioactive method for determining the age of minerals appears to be thoroughly reliable. It is applicable mainly to igneous rocks, and gives the time since they solidified. The geological age of such rocks can usually be determined by a study of the neighboring strata. In this way many points of the geological sequence can be dated, and the agreement with the order of succession indicated by the stratigraphic and paleontological evidence is admirable.

We are concerned to-night with the greatest ages this revealed, and these run up to 1,500 or even 1,800 million years. The most striking example is a pegmatite in Manitoba, in which have been found crystals of uraninite containing uranium, of monazite containing thorium, and of mica containing rubidium. This is also radioactive, and breaks down very slowly into an isotope of strontium. The ages determined independently from these three minerals and based on different radioactive processes, come to 1,600, 1,900, and about 1,700 million years.

[3] In the customary American notation, 10^9 years, not 10^6 or 10^{12}.

[4] A. O. Nier, *Phys. Rev.*, 55: 153, 1939; A. F. Kovarik and N. I. Adams, *Phys. Rev.*, 53: 928, 1938.

There seems to be no escape from the conclusion that these minerals had actually been lying undisturbed in the rock for more than a billion and a half years, until the miner blasted them out. The Earth must be at least as old as this.

The same radioactive processes may be used to find an upper limit to the age of the Earth, or at least of its crust. Radioactive tests are so delicate that we can find, with considerable accuracy, the amounts of uranium and thorium which are present per ton of rock, and hence, taking an average of rock-types, for the upper crust of our planet. The amount of lead must be found by ordinary chemical methods, but is fairly well estimable. Now, so long as the Earth's crust *as a whole* has not been mixed with other matter, the lead produced by the decay of the uranium and thorium must still be in it. A simple calculation shows that all the existing lead would have been produced radioactively in between three and four billion years. This figure is considerably less accurate than the last; but it should represent a true upper limit, as there was very probably "ordinary" lead present. Provided that the amounts of uranium, thorium and lead are well determined, this calculation holds, no matter how many times the material of the crust may have been melted or re-worked, so long as no process selectively removed the uranium or the lead into the inaccessible interior of the earth. It appears, then, that the earth's crust is more than $1\frac{3}{4}$ and less than $3\frac{1}{2}$ billions of years old.

That so great a time-interval can be determined within a factor of two is remarkable.

If all the helium which is produced by radioactive changes within a mineral remained in it, measurement of its amount would serve to determine its age. If some of the gas escapes—as it is likely to do except from dense crystals or fine-grained rocks—the calculated age will be too low. This method, applied to terrestrial minerals, gives ages comparable with the lead-ratio methods, or lower.

Paneth has employed it upon meteorites, finding low values in some cases (as might be expected) and maximum ages up to 2,800 million years for others. The recent researches of Whipple at Harvard show that several bright meteors which have been accurately observed by photography were moving around the sun in asteroid-like orbits before they hit the earth. These meteorites may then be regarded as members of the solar system—though there is a chance that a small percentage were visitors from interstellar space.

The age of the earth appears therefore to be substantially the same as that of the solar system as a whole (so far as we can test it).

At this point we lose the powerful aid of chemical analysis. Theoretically we might hope to detect uranium, thorium and lead spectroscopically in the Sun, or even the stars. Lead has actually been found; but unfortunately the character of the spectra of uranium and thorium, which contain great numbers of rather weak lines, makes the spectroscopic test in the Sun relatively insensitive, and no evidence is available.

(2) A determination of time-scale of a radically different sort may be made from observations of the extra-galactic nebulae (or external galaxies, as some prefer to call them). The brilliant work of Hubble has proved that these objects are enormous clouds of stars, thousands of light-years in diameter, and, except for the very nearest, millions of light-years away. The distances of the nearer ones can be determined satisfactorily, by a study of the brightness of individual stars within them, especially of variable stars of the Cepheid type, and in this way Hubble has found the distribution of size and brightness among the members of the nearest of the great clusters in which these nebulae often congregate. On the reasonable assumption that the average brightness and the range about the average is similar from one cluster to another, the distances of faint clusters can be derived, some of them as great as 300 million light-years.

The spectra of these nebulae are what might be expected from a cloud of stars; but their spectral lines show a "red-shift" which increases steadily with increasing faintness and distance. This shift is of exactly the kind which would be produced by the Doppler effect due to a very rapid recession. The velocities thus derived are far greater than any previously known, and attain the enormous value of 42,000 kilometers per second for members of a cluster in Ursa Major, whose distance is estimated at 240 million light-years. The observations of these spectra, which have been made mainly by Humason, demand the 100-inch telescope, a specially designed spectrograph of very great light-power, and great skill and assiduity. Spectra have been photographed for nebulae too faint to be directly seen even with this greatest of telescopes—the necessary guiding being done with the aid of a nearby star, whose position, relative to the nebula, had been measured on long-exposure photographs and allowed for.

Hubble has shown conclusively that, in general, the velocity of recession of a nebula is proportional to its distance, a nebula one million light-years away having a velocity of 162 km/sec, etc. There are moderate outstanding differences for the separate nebulae, which presumably represent their "peculiar" or individual motions; but these do not at all obscure the general trend.

This proportionality between velocity and distance may be expressed as follows. If the nebulae could all have been collected at one point at a certain time in the remote past, and each one launched into space in its present direction, and with its present speed, they

would all, at the present time, have reached their observed positions and distances. We can not, of course, be sure that their actual motions have continued at a uniform rate; but the assumption that they have gives a time-scale appropriate to the description of these motions. Making the simple calculation we find that the hypothetical starting-time comes to 1,840 million years ago.

The agreement of this with the calculated age of the earth or of the solar system is very striking, since it is absolutely unforced. The two calculations have no data whatever in common, and their results might just as well have disagreed by a factor of ten, or even a hundred, as have agreed, as they do, within a factor less than two.

The interpretation of these red-shifts leads us far into the field of general relativity. According to this space itself, if it contains matter, will usually not be in a steady state but may expand or contract—the matter moving, too. The observed red-shifts are explicable if the universe is now expanding. Many different courses of change are mathematically possible: Space may have once been much smaller than at present, with the stars (or whatever they were if they had not yet begun to shine) close together; or it may have expanded, at first slowly, then faster, from a finite original size smaller than at present. In the future it may continue to expand indefinitely, or reach a maximum "size" and then contract again. With our present limited knowledge, we can not rule out any of these possibilities. When certain data regarding the numbers and distribution of nebulae down to the faintest limit observable with the 200-inch telescope become available, it may be practicable to specify which type of solution fits the more accurately known facts.

In the meantime, the most interesting solution is that of Lemaître, according to which the whole universe was once packed into a narrow compass. He goes so far, a little speculatively, as to envisage a state in which all matter formed one gigantic atom, which broke up and threw off the raw materials for millions of galaxies, and millions of billions of stars. The principal attraction of this scheme is that it pictures a short but tumultuous time in the early days of our present universe, during which all sorts of things which never can happen now might have occurred—such as the origin of the planetary system, and—as we shall see later—the formation of the heavy radio-active atoms.

The interval since these "fireworks," as Lemaître once called them, should be of the order of the 1,800 million years already calculated—perhaps shorter. This makes the earth and all parts of the visible universe contemporaries, and less than two billion years of age.

(3) Objections to this "short" time-scale have been raised by various investigators. Some have proved to be unfounded, but one, suggested a dozen years ago by Jeans, deserves our consideration.

The orbits of binary stars of long period (decades or centuries) are usually highly eccentric, while those of close pairs with periods of months or days are usually nearly circular. So long as a double star remains isolated in space, the relative orbit of the components will be unaltered. If another stray star should pass near it, but at ten times the distance of the close pair, its attraction, being somewhat greater on one component than on the other, would alter the orbit, but not seriously. But if the intruder passed nearer to one of the stars than its companion was at the time, the perturbation due to its attraction would be large and might produce great alterations in the orbit. Sometimes the new orbit would be more eccentric than the old, and sometimes less: But Jeans has shown that the net effect of a great number of such encounters would be to make the average orbital eccentricity 0.66, with a wide range in the individual values.

The actual mean eccentricity for pairs whose orbits have been computed, which can usually be done only if their periods are less than three or four hundred years, is about 0.5, but there is evidence that for the wider and slower moving pairs it increases to about Jeans's value.

For short periods of months or less, the intruding star would have to come far closer to alter the orbit to the same degree. Such close approaches will evidently be much less likely than the wider ones which are effective for the slow pairs, hence, given time enough and not too much, the orbits of the wide pairs should be pretty well "knocked about" while the close pairs are still little affected.

With the present density of distribution of the stars in space, and their present velocities, this production of eccentric orbits should be an exceedingly slow process. Jeans estimates that to get it well toward completion would require 5 trillion years (5×10^{12}).[5]

This leads to a time-scale a thousand times as long as the one we have previously met. The argument is based on sound mathematical analysis, but also on an assumption—that the binary orbits got their high eccentricities in this way. It has been completely upset by the application of just the same reasoning to another class of stellar systems, namely, the moving clusters.

The star-group of the Hyades in Taurus—recognized and known by this name since classical antiquity—contains about 300 stars distributed within a region some forty light-years in diameter, which are moving together in space, in the same direction, and at the same rather rapid rate. Such community of motion

[5] "Astronomy and Cosmogony," p. 322. Cambridge, 1928.

can not possibly be an accident. No one doubts that the cluster stars have a common origin, and that they have been moving together indefinitely long in the past. The attraction of the stars on one another must hold the cluster together, but this is so small that if the cluster were isolated in space its outer stars would take 50 million years or more to revolve around the center.

Expressed otherwise, it is only about a millionth part of the attraction which the components of a wide binary pair (with separation 100 times that of the earth from the sun) exert upon one another. The disturbing force due to a star passing by at a fairly considerable distance, although small compared with the mutual attraction of a binary pair, would often be great enough to set the cluster star (or the binary pair together) in motion at such a rate that it would escape entirely from the attraction of the cluster and be lost to it.

This is effectively an irreversible process. One interloping star *might* meet another interloper under such circumstances that after their encounter one of them was left moving so slowly relative to the cluster's center that it would be "captured"; but the probability of this is infinitesimally small.

All moving clusters must therefore be gradually disintegrating as their members are removed, one by one. The process is slow; but it must be far more rapid than the alteration of the far more firmly bound binary orbits by similar encounters. Hence, if the latter had taken place to any considerable extent, sparse clusters such as the Hyades, and denser ones like the Pleiades, too, would have been completely disrupted into apparently unrelated stars.

The clusters are there in the sky; and some, such as the Hyades, contain typical binary stars with eccentric orbits. The conclusion is unescapable that the double stars have not had enough knocking about to produce the eccentricities of their orbits (which must have originated in some other way not yet understood) and hence that they have not been subject to encounters for anything like the trillions of years which Jeans suggests.

A detailed analysis by Bok[6] shows that there is a second force tending toward the disintegration of a moving cluster, which is usually more effective than the chance encounters already discussed, namely, the tidal effect due to the attraction of the great mass of stars at the center of the galaxy. He calculates that the Hyades will apparently resist this influence, without much change, for two billion years to come, but that, within a billion years more, the cluster will be completely dissipated. A denser and more massive cluster like the Pleiades should last ten times as long.

These numerical calculations are based upon data entirely independent of any which entered into the earlier discussions. The analysis deals this time with the future, not with the past, but once more it leads to a time-scale measured in billions of years.

(4) There is still one more time-scale which we have to consider—that of the life of a star as a luminous body. The main principles of this study were developed by Eddington between fifteen and twenty years ago. A large mass of matter in space—anything more than 100,000 times as massive as the Earth—must have an enormous pressure in its interior, owing to its own gravitation. To withstand this pressure without collapse, the material must be at a very high temperature, millions of degrees, except for a few stars of exceptionally great size. Hence, the matter must be gaseous and highly ionized, with most of the electrons knocked off the atoms. The properties of matter in this state are much simpler than in any other, so that a general theory of the internal constitution of the stars becomes possible.

Since the inside is hotter than the surface, heat must leak out from the interior down the temperature gradient; and it is this which keeps the stars shining. The opacity of the gas can be determined by modern atomic theory, and hence the total energy radiation from the star's surface—its luminosity—may be calculated. It is found that the luminosity increases very rapidly with the star's mass—rather faster than its fourth power, on the average. For the same mass, it changes but slowly with the star's size (inversely as the square root of its radius). Differences in the internal density distribution—the "model"—make surprisingly little difference in the luminosity, hardly more than one stellar magnitude. The chemical composition makes little difference, too, except for the abundance of hydrogen. A mass of almost pure hydrogen is cooler than one composed mainly of heavy atoms, and its luminosity will be less by fully six magnitudes—a factor of 300.

Applying this to the sun, Strömgren finds that the calculated and observed luminosities agree if hydrogen forms 36 per cent. by weight, of the interior mass, the rest being heavy elements.

The principal point of all this for us now is that a large mass, isolated in space, has *got* to shine, to be a star, and to disperse enormous stores of energy into the unknown depths of space.

The only source known to the older physics from which this could be derived is the gravitational energy of the star itself. A steady contraction, too slow to be detectable within the few centuries of exact observation, would draw from this enough to keep the sun shining. But the whole store of energy which could have thus been available to the sun in the past is easily calculable; and it is only enough to supply the present rate of radiation for 15 million years.

[6] Harvard College Observatory *Circular* No. 384, 1939.

Here is a short time-scale with a vengeance. There must be some way out; and this way, suggested on general principles by various astrophysicists twenty years ago, has just been established on a basis of experimental physics. Relativity predicts that mass and energy should be interconvertible; and many nuclear reactions have been observed in the laboratory in the last few years in which the diminution of the combined masses of the interacting nuclei, and the appearance of the corresponding amount of energy, have been observed. The great energy-liberating process is the building up of hydrogen into heavier elements. The transformation of four atoms of hydrogen into one of helium, for example, diminishes the mass by 1 part in 135.

Now the sun's energy-radiation is known, and it corresponds to a loss of mass of 4,200,000 metric tons per second, corresponding to the transmutation of 570 million tons of hydrogen into helium per second. This is a prodigious amount, but, at 36 per cent. of the whole mass, there is enough hydrogen in the sun to last 40 billion years at this rate—so that we now get a rather long time-scale.

The process by which this transmutation takes place has been identified in detail by the theoretical and experimental work of Bethe. Hydrogen nuclei (protons) colliding with a carbon nucleus, under suitable conditions, build it up into the heavier isotope. Collisions with this again build first the lighter, then the heavier isotope of nitrogen, and collisions with the last split off an alpha particle (helium) and leave a carbon nucleus ready to begin again. Electrons or gamma rays are given off at every stage of the process and carry the released energy into the surrounding gas.

Every stage of this series of atomic events has now been studied in the laboratory, and it is possible to calculate at what rate the cycle would occur in the gas inside the sun for a given temperature, pressure and composition. Assuming 35 per cent. of hydrogen and 1 per cent. of the heavier elements (which accords well with the intensities of lines in stellar spectra) it is found that enough heat to keep the sun going would be liberated if the temperature at its center was 19 million degrees. This is very close to the value of the central temperature which had previously been derived from the sun's observed size and mass, and the assumed hydrogen abundance.

We may therefore fairly claim that we know just why the sun shines, and can be reasonably sure that it will keep on shining for at least 10 billion years in the future (not forty, because it should grow brighter as the hydrogen gets used up). How long it has been shining in the past depends on how much hydrogen was "originally" there when it started as an independent body. If it was almost all hydrogen then, this would have been some 80 billion years ago, but this is an extreme limit.

Bethe's theory accounts not only for the sun, but for the whole great main sequence of stars, which extends from great hot white stars like those in Orion through Sirius and Procyon to the sun and to the faint red dwarf stars.

From the bright end of this sequence to the faint, we come upon stars of smaller and smaller mass, and also smaller diameter. The central temperatures may be as high as 35 million degrees at the upper end, and as low as 12 million at the bottom. The rate at which the transmutation process works increases enormously with the temperature, and the differences just described are very nearly what would be needed to provide the great radiation from the massive stars, and the feeble luminosity of the small ones.

This theory accounts for the present properties of all these stars, and enables us to predict their future; but it tells us little about their past. If, in any way, bodies of composition resembling the sun's, and of all sorts of masses, were scattered here and there through space, each one would settle down into a star. If its central temperature was at first too low to "turn on the heat" from atomic transformation, it would draw on its gravitational energy and contract till the interior became hot enough to make this work. After a few millions of years doing this, it would settle down to a steady life of billions of years, gradually consuming its hydrogen. The gravitational contraction would last longest for the least massive stars, which radiate feebly; but, after a few hundred million years, all trace of this adolescent stage would be lost.

So far, all is well; but there are still a number of acute and unsolved problems. First comes the visible existence of stars of very high luminosity. For example, Y Cygni, near the top of the main sequence, has seventeen times the sun's mass, and about 30,000 times its luminosity. It must therefore be living its life nearly 2,000 times as fast as the sun. For 29 Canis Majoris, at the very top, the mass is 46 and the luminosity is estimated by Chandrasekhar as 700,000, so that it is living more than 10,000 times as fast as the sun. The transformation of its whole mass from hydrogen to helium would supply this rate of shining for only about 10 million years, while Y Cygni might last 60 million.

Many other stars are known which are many thousands of times as luminous as the sun, and must be living their lives hundreds of times faster; and among these are red super-giants of enormous diameter like Zeta Aurigae. If these are built at all like the sun inside, their central temperatures can not be much over a million degrees. The self-regenerating carbon cycle would not work at all at these temperatures—the only

hope seems to be in the interaction of deuterons (heavy hydrogen) and protons (or for somewhat higher temperatures, of lithium, etc.). These elements are very rare on earth, and in the sun. We can not prove that they might not have been more abundant in these stars. But all known nuclear reactions which involve them use them up, and, whatever assumptions we choose to make, the difficulty of the short time-scale remains. Milne's suggestion (which can not at present be disproved, though it does not look likely) that these giant stars have small dense hot nuclei in which other nuclear reactions are at work, only reduces the difficulty to the same level as for the hotter stars.

Here we are faced with a difficulty which is not yet resolved. Perhaps these stars began their existence as luminous bodies at a relatively late date in the billion years or more of history which other evidence assigns to the sidereal universe. In that case, it is hard to explain in what physical state the matter which composed them previously was, and what prevented it from starting to condense. Moreover, where are the stars which it is reasonable to suppose started earlier and have exhausted their hydrogen?

Again, it is conceivable that inside these stars the far more drastic hypothetical process of the complete transformation of mass into energy may take place—the annihilation of matter. Since nothing of the sort has been observed in the laboratory (saving the mutual annihilation of positive and negative electrons, which is beside the point) we are here in the realm of the purely speculative.

A second puzzle is raised by the existence of the white dwarfs—stars of low luminosity and enormously high density. More of these are being discovered every year, and it is evident that they form the most numerous of all classes of stars, excepting the red dwarfs, if the count is made, as it should fairly be, in a given volume of space.

Our theoretical understanding of these stars is the most satisfactory that we possess. They represent senility—the approach to the final state in which all the available energy, gravitational, nuclear, or what not, has been exhausted and radiated away into space, and nothing more can happen. Within them, the electrons are degenerate—jammed together as closely as the quantum laws permit, and further contraction is impossible.

Chandrasekhar has shown that the radius and density of a star in this state are determined by its mass (and the amount of hydrogen remaining in it). The greater the mass, the smaller and denser will be the star. For masses less than a certain limit (1.4 times the sun's mass, if no hydrogen is present) the process of contraction will end in a degenerate state. For larger masses the critical conditions will not be reached, and so far as our present knowledge goes, a star may contract indefinitely.

Now to reach the white-dwarf stage, a star like the companion of Sirius, which has nearly the same mass as the sun, must have gotten rid of all its stores of energy. If it started like the sun, it would require many billions of years to do this, while white dwarfs of smaller mass (o^2 Eridani B) which live their lives more slowly, will take still longer. When we find numbers of such stars in an expanding universe which looks as if it were only a billion or two years old, we are moved to drop into the vernacular and ask "How did they get that way?"

A bold answer was given by de Sitter. May it not be that these dense stars *came through* the period when space was last small, so that they are older than the galaxies? They would be tough nuts to crack; everything depends on how great the turmoil was at the critical time.

Finally, one more puzzle is "How did the radioactive elements get there?" These are certainly decaying, and, when bombarded in the laboratory, they break up all the more. To build them up out of lighter atoms would probably demand extreme conditions of temperature and pressure, such as can not be reached in the hotter stellar interiors.

It was supposed, a few years ago, that the liberation of energy by transmutation of hydrogen might result, as a by-product, in the formation of the heavier elements. But a careful investigation by Bethe indicates that this is extremely improbable—at least at such temperatures as exist inside the stars. So long as hydrogen and carbon are both present, this temperature can not rise much above 30 million degrees. Otherwise there would be so rapid an evolution of energy as to force a reversal of the process of gravitational contraction, expanding the star, and making its interior cooler. No way for the formation of sodium and heavier atoms appears to be open. But these elements exist in considerable abundance on the surfaces of the stars, and it is probable that they form a large part of the 64 per cent. of the sun's interior which is not hydrogen.

How they got there we do not know. It is tempting to think that just before the last expansion of the universe started, much, if not all, of its matter was under great pressure and at an exceedingly high temperature; but this is speculative.

The outcome of our discussion is this. A time-scale for the universe measured in billions of years—and in very few of them—is indicated by four independent lines of evidence. The outstanding difficulties are to explain why the giant stars still shine, and why the white dwarfs are already present.

Above all, all the witnesses proclaim with one voice

that the present order of things is transitory. As Eddington puts it, "the stars are in their first innings." If things go on as they are, in less than a hundred billion years the spiral nebulae will have receded out of sight, the radioactive atoms will have run down, all but the fainter stars will be going-out—and the universe will be thoroughly uninteresting.

Of course by that time—or perhaps later—the expansion of the universe may give place to a contraction, which continues till everything—including the radiation now in remote depths—has been crowded together again in small compass; and the universe may start afresh.

Whether this may be so or not we have no knowledge; but it is surprising to me how strong an aesthetic hold this conception has on many people. (I say deliberately aesthetic and not religious, for religion has never concerned itself much about the fate of material things.)

With this wide-spread desire to believe in some cyclical restoration of activity at however great intervals, I admit frankly that I am not in sympathy. I agree with Eddington, "I am an evolutionist, not a multiplicationist. It seems so tiresome to be doing the same things over and over again." But it is in other words that I would leave the expression of this attitude with you—Rupert Brooke's:

There are waters blown by changing winds to laughter
And lit by the rich skies, all day. And after
Frost, with a gesture, stays the waves that dance
And wandering loveliness. He leaves a white
Unbroken glory, a gathered radiance,
A width, a shining peace, under the night.

SCIENTIFIC EVENTS

THE NEW CHEMICAL LABORATORY BUILDING OF THE UNIVERSITY OF PENNSYLVANIA

Plans to begin construction during the bicentennial year of the University of Pennsylvania of the first of three units of the new Chemical Laboratory Building have been announced.

Dr. Paul P. Cret, professor emeritus of design, architect and alumnus, has been authorized to draw complete specifications for the first unit and plans for the two additional units to be added as funds become available. For construction and endowment of the entire building the sum of $2,000,000 is required.

The present John Harrison Laboratory of the university was established by gifts from the late Provost Charles Custis Harrison and his brothers as a memorial to their grandfather, who founded in Philadelphia, in 1792, the first permanent chemical plant in this country. The laboratory was first occupied in the fall of 1894, the department of chemistry having previously been quartered in College Hall.

In 1894 the registration of students in the department of chemistry was 57. Now there are 450 students. This number represents an increase during the past seven years of more than a hundred per cent.

Including students enrolled in other schools or departments and not majoring in chemistry, there are now more than 3,000 in the courses in chemistry and chemical engineering.

The building is one of many advances made possible by the more than 15,000 alumni and friends who have given to the Bicentennial Fund the sum of more than $4,300,000. There will be presented to the university on September 20 a Bicentennial Honor Roll containing the names of all alumni, alumnae, students, friends, firms and corporations, foundations and other organizations contributing to the fund up to that time. It will be placed with other memorabilia of the times in a sealed packet, to be preserved unopened until the year 2040, which will be the three-hundredth anniversary of the university.

THE VIRGINIA JUNIOR ACADEMY OF SCIENCE

According to Dr. E. C. L. Miller, secretary-treasurer of the Virginia Academy of Science, there are now fifty-three organized science clubs in the secondary schools of the state of Virginia, sponsored by teachers in the various schools. Some forty-five more are in the formation period. Steps have been taken to organize these clubs into a Junior Academy of Science. At a meeting on June 5 two committees were appointed for this purpose; the members of the first committee to function as officers of the Junior Academy for the rest of this year, the second committee to function as an advisory committee from the senior academy, with final organization plans to be made at the Richmond meeting of the academy next spring.

Members of these committees are:

Virginia Junior Academy of Science

H. J. Davis, *chairman*, Pocahontas.
W. W. Nofsinger, *vice-chairman*, Jefferson Senior High School, Roanoke.
Miss J. Frances Allen, *secretary*, Alfred Belle Apartments, Pulaski.
J. T. Christopher, George Washington High School, Danville.
C. G. Gibbs, Floyd High School, Floyd.
Miss E. Gillespie, Maury High School, Norfolk.
Wm. T. Hall, Clarksville High School, Clarksville.

H. S. Holmes, Petersburg High School, Petersburg.
Miss Martha Lipscomb, Thomas Jefferson High School, Richmond.
W. I. Nickels, Jr., Lane High School, Charlottesville.

VIRGINIA ACADEMY OF SCIENCE SPONSORING

Professor J. A. Rorer, *chairman*, director, Extension Division, University of Virginia.
Dr. George W. Jeffers, *vice-chairman*, State Teachers College, Farmville.
L. F. Addington, Wise.
L. C. Bird, president, Phipps and Bird, Inc., Richmond.
Francis S. Chase, *executive secretary*, Virginia Education Association, Richmond.
Dr. I. A. Updike, Randolph-Macon College, Ashland.

The junior academy will meet with the senior academy at the regular annual meeting, have a program of its own and present exhibits of the work done by members of the science clubs. It is hoped also to have exhibits at the Thanksgiving meeting of the Virginia Education Association in Richmond. The senior academy plans to foster and assist these science clubs in any way it can and to encourage the development of more clubs.

Members of the junior academy will eventually become members of the senior academy, and will gradually take their places as leaders in the scientific life of the state.

SUMMER WORK IN BOTANY OF THE UNIVERSITY OF MICHIGAN

THIS year, owing to international conditions, members of the faculty of the University of Michigan, according to the *Alumni Journal*, are confining their activities largely to work in the United States. The summer camps include the Biological Station, Douglas Lake; Camp Davis, Jackson Hole, Wyo., where work is being done in surveying, geology and botany; Camp Filibert Roth, the forestry station at Golden Lake, and the camp at Wilderness Park, near Mackinaw City, where a field course in geography is being offered.

Members of the department of botany are engaged in trips to areas from Alaska to Panama. Professor Harley H. Bartlett, department chairman, and Tobias Lasser have gone to the Chagres River, Panama, for malaria investigations; Professor D. V. Baxter will make a collecting trip to Alaska. Other collectors include Professor F. M. Pagan, who will collect in Mexico; Professor C. A. Arnold, in Colorado; Professor B. M. Davis, in Oregon; Dr. J. L. Baldwin, Jr., in the mountains of West Virginia, North Carolina, South Carolina and Georgia, and Dr. E. U. Clover, in the Havasupai Canyon, Ariz. Professor W. R. Taylor will be at the Woods Hole Marine Biological Laboratory. After spending the college year at the University of Puerto Rico, where he collected mosses and liverworts, Professor W. C. Steere will devote the summer to work at the Biological Station at Douglas Lake. Professor L. E. Wehmeyer will be at Camp Davis.

Members of the Forestry and Conservation School are doing experimental work at the Chase S. Osborn Preserve at Sugar Island and at "Ringwood," near St. Charles. Professors W. F. Ramsdell, L. J. Young and others are engaged in this study. In addition, Professors E. C. O'Roke and S. A. Graham are engaged in research at and near Camp Filibert Roth; Professor D. M. Matthews is carrying on research on the relation between logging costs and forest management on the Pacific coast and in the "Inland Empire"—Idaho, Washington and Montana; Professor S. W. Allen is leading two expeditions for the American Forestry Association in the Rocky Mountain region, and Professor D. V. Baxter is conducting a study and collection of fungi in Alaska.

From the University Herbarium, Dr. E. B. Mains is studying the fungi of the Colorado mountains; Dr. and Mrs. C. L. Lundell, the flora of Texas, and Dr. A. H. Smith, Michigan fungi.

THE BROWN UNIVERSITY ECLIPSE EXPEDITION

AN attempt to prove definitely that the zodiacal light of the sun can be photographed during a total eclipse will be made by an expedition to South America headed by Professor Charles H. Smiley, chairman of the department of astronomy of Brown University, to observe the eclipse of October 1.

The expedition, which is sponsored also by "the Skyscrapers," an amateur astronomical society of Providence, R. I., will set up high-speed cameras near the village of Quixeramobim, Brazil, at the eastern shoulder of South America about 100 miles northwest of Pernambuco. At this point the total eclipse will occur at 10 A.M., and will last for nearly five minutes.

Accompanying Professor Smiley will be Mrs. Smiley and Arthur A. Hoag, of Barrington, R. I., a junior at the university. Dr. Alice Farnsworth, head of the department of astronomy at Mount Holyoke College, may join the group in South America. Good weather is anticipated, since it seldom rains or is even cloudy in Quixeramobim in October.

Although only three other astronomers have reported seeing the zodiacal light during an eclipse of the sun, Professor Smiley photographed what he describes as "similar phenomena" during the total eclipse of June 8, 1937, when he took pictures from a vantage point high up in the Peruvian Andes. These showed a double wedge-shaped light area fanning out from above and below the sun, and extending at least 25 times the diameter of the sun into space. It was cen-

tered along the ecliptic. The phenomenon was distinctly separate from the corona. Professor Smiley hopes to verify these results, and to obtain more conclusive photographs on his forthcoming expedition. He is especially interested in determining whether the zodiacal light can be seen and photographed at sea level. Other observations, including his own in Peru, have been made from altitudes of at least 14,000 feet.

Two astronomical instruments recently devised, the coronograph and the coronavisor, now permit astronomers to view the bright inner corona of the sun at any time, but an eclipse is still necessary to see the outer corona and the fainter zodiacal light.

The expedition plans to leave Providence about August 15. With the European war eliminating many steamer routes, it will probably take a boat for Rio de Janeiro, and then retrace its route *via* the coast of Brazil, either by land, air or water, to Quixeramobim.

CORRESPONDENT

THE DETROIT MEETING OF THE AMERICAN CHEMICAL SOCIETY

THE American Chemical Society will meet in Detroit from September 9 to 13. All divisions will be represented on the program. According to the announcement of the society nineteen symposia have been planned.

The Division of Agricultural and Food Chemistry is sponsoring a symposium on Fruits and Fruit Products and another on Animal Nutrition, the latter emphasizing phases other than vitamin chemistry. It is joining with the Division of Fertilizer Chemistry in celebrating the one hundredth anniversary of Liebig's paper on Agricultural Chemistry. The papers to be given will evaluate the significance of Liebig's contributions in the light of modern agricultural chemical concepts. The Division of Biological Chemistry has organized a program on vitamins and nutrition in which the Division of Agricultural and Food Chemistry will cooperate. Two sessions will be held for presentation of papers on miscellaneous subjects within the field of the division.

The Division of Biological Chemistry is planning symposia on phases of the protein problem; tentatively, titles selected are Aspects of Intermediary Protein Metabolism and Aspects of Sulfur and Protein Metabolism. The usual program on vitamins and nutrition will be held jointly with the Divisions of Agricultural and Food Chemistry and Medicinal Chemistry.

The Division of Cellulose Chemistry will meet for three general sessions.

The Division of Chemical Education in two symposia will discuss the first two years of college chemistry and chemistry in the high school.

The Division of Colloid Chemistry will present a program of diversified papers.

The Division of Fertilizer Chemistry will sponsor a symposium on Liebig in which his contributions to agricultural and fertilizer chemistry will be discussed. This program commemorates the one hundredth anniversary of Liebig's epoch-making address before the British Association for the Advancement of Science. The Division of Agricultural and Food Chemistry will join in this celebration. A program of general papers will be offered.

The Division of Gas and Fuel Chemistry has arranged two sessions of miscellaneous papers and a symposium on Atmosphere Conditioning for Metallurgical and Chemical Processes.

The Division of the History of Chemistry will offer a short program of miscellaneous papers.

The Division of Industrial and Engineering Chemistry has organized three symposia: Glass—What Is Old? What Is New? under the chairmanship of Alexander Silverman; New Textile Fibers, Fabrics, and Finishes under the leadership of Gustavus J. Esselen, and Unit Processes under the direction of R. Norris Shreve.

The Division of Medicinal Chemistry will cooperate with the Divisions of Agricultural and Food Chemistry and Biological Chemistry in a program on vitamins and nutrition. It also is sponsoring a symposium on Methods for the Standardization of Drugs and a program of general papers.

The Division of Microchemistry will meet for two sessions of miscellaneous papers.

The Division of Organic Chemistry will hold six sessions for the reading and discussion of papers.

The Division of Paint and Varnish Chemistry is arranging one session for papers on resins and plastics, two on paint and varnish and another symposium on Automotive Finishing.

The Division of Petroleum Chemistry is featuring an extensive symposium on Petroleum Chemistry's Contribution to the Automotive Field. This will not only include the better known uses for power and lubrication, but will consider uses of petroleum products in coatings, plastics, shock absorbers, etc.

The program of the Division of Physical and Inorganic Chemistry includes three symposia: The Transfer of Energy in Molecular Collision; Chemical Equilibrium at High Pressures and Elevated Temperatures, and Fluorine Chemistry. Sessions will be available for papers on physical, inorganic and analytical chemistry. The division will meet, as usual, for dinner.

The Division of Rubber Chemistry is sponsoring a symposium on Rubber in the Automotive Industry to be held on Thursday. Ample opportunity will be provided on the following day for the presentation of other papers on rubber chemistry. The division will hold its banquet on Thursday evening in the Book-Cadillac Hotel. W. J. Cameron, of the Ford Motor Company, will be the speaker.

The Division of Sugar Chemistry and Technology will have a program of miscellaneous papers in two sessions.

The Division of Water, Sewage and Sanitation Chemistry plans three general sessions. On Thursday the members will visit the Springwells Filtration Plant, the Detroit Sewage Plant and the Dearborn Sewage Plant. Lunch will be served at the Dearborn Inn, followed by a short visit to Greenfield Village.

SCIENTIFIC NOTES AND NEWS

THE National Academy of Sciences will meet on October 28, 29 and 30 at Philadelphia following the bicentennial celebration of the University of Pennsylvania. Sessions will be held in the Zoological Laboratory. On Monday evening there will be a public lecture in the University Museum, following a dinner given by the university for the members of the academy. On Tuesday evening there will be a reception by President and Mrs. Jewett and a subscription dinner at the Bellevue-Stratford Hotel.

PROFESSOR RAYMOND PEARL, of the Johns Hopkins University, has been elected an honorary member of the Royal Society of Medicine (Section of Epidemiology and State Medicine).

AT the convocation of the University of Chicago on June 11 the honorary degree of doctor of science was conferred on Dr. Ludvig Hektoen. The citation reads: "In recognition of his scholarly achievements in basic science and his distinguished service to the university and to medicine as a great teacher, editor and administrator."

AT the commencement exercises of the North Carolina State College at Raleigh the honorary degree of doctor of engineering was conferred on Dr. Clement L. Garner, chief of the Division of Geodesy of the U. S. Coast and Geodetic Survey.

THE Osler Memorial Medal of the University of Oxford for 1940 has been awarded to Sir E. Farquhar Buzzard, professor of medicine in the university.

DR. ANDREY ABRAHAM POTTER, dean of the Schools of Engineering and director of the Engineering Experiment Station at Purdue University, was presented with the Lamme Medal for 1940 of the Society for the Promotion of Engineering Education at the recent meeting at the University of California. Presentation of the award was made at the annual dinner by Professor Herbert B. Langille, of the university. The citation reads: "for his leadership in the advancement of the profession of engineering; for his devotion to high standards of teaching and his contributions to the development of engineering education; for his understanding of human nature and sympathetic interest in the work of his associates and students, for his sound judgment and skill as an engineer, and for his untiring efforts in developing cooperative relations between engineering colleges and industry."

THE medal of the American Laryngological, Rhinological and Otological Society for "conspicuous public service" has been awarded to Dr. James Sonnett Greene, founder and director of the National Hospital for Speech Disorders, "for his unselfish devotion to the alleviation of speech defects in those thus afflicted, and for the success which has crowned his life-long efforts in having established the National Hospital for Speech Disorders."

THE James Alfred Ewing Medal of the British Institution of Civil Engineers, founded in 1936 in memory of the late Sir Alfred Ewing and given "for especially meritorious contributions to the science of engineering in the field of research," has been awarded for 1939 to Professor G. I. Taylor, Yarrow research professor of the Royal Society.

DR. HERBERT A. WAGNER, president of the Consolidated Gas, Electric Light and Power Company of Baltimore, has retired from the presidency of the Maryland Academy of Sciences and is succeeded by Dr. S. Karrer, who has been director of research of the company since 1926. Dr. Wagner continues to be chairman of the board of trustees.

Museum News reports that on March 26 at the annual meeting of the South African Museums Association at Capetown, E. C. van Hoepen, of the National Museum, Bloemfontein, was elected president, and E. C. Chubb was reelected secretary and treasurer. The association voted to hold the meeting in 1941 at Johannesburg.

AT the Harrison, N. Y., meeting of the American Neurological Association officers were elected as follows: Dr. H. Douglas Singer, of Chicago, president, and Dr. Gilbert Horrax and Dr. H. C. Solomon, both of Boston, vice-presidents.

AT the recent annual general meeting of the British Institute of Physics the following officers were elected to take office on October 1: *President,* Professor W. L. Bragg; *Vice-president,* Dr. B. A. Keen; *Honorary Treasurer,* Major C. E. S. Phillips; *Honorary Secretary,* Professor J. A. Crowther; *Members of the Board,* Professor E. A. Owen, Dr. C. Sykes and Professor G. I. Finch (appointed by the Physical Society), and Dr. R. W. Lunt (appointed by the Faraday Society).

PROFESSOR OTTO L. KOWALKE retired on July 1 from the chairmanship of the department of chemical engineering of the University of Wisconsin. He has been chairman of the department for twenty-seven years and a member of the teaching staff for thirty-three years. He plans to devote his time to research and teaching.

PROFESSOR A. B. DAWSON, who has been director of the Biological Laboratories of Harvard University for the past five years, has been appointed chairman of the department of biology to succeed Professor F. L. Hisaw, who recently resigned.

Dr. Milton J. Rosenau, head of the Division of Public Health of the Medical School of the University of North Carolina, the title of which has been changed to the School of Public Health, becomes first dean of the new school.

Dr. Bruno Rossi, formerly professor of physics at the University of Padua, research associate at the University of Chicago, has been appointed associate professor of physics at Cornell University.

Dr. Meredith P. Crawford, formerly instructor in psychology at Barnard College, has been appointed assistant professor of psychology at Vanderbilt University.

Dr. Hoke S. Greene, director of graduate studies in chemistry and chemical engineering at the University of Cincinnati, has been made associate professor of chemical engineering; Harold J. Garber, who took part in the planning and construction of the new addition to the chemistry building, has been appointed assistant professor in chemical engineering.

Dr. Charles C. Deam, research forester in the Division of Forestry of the Conservation Department of the State of Indiana, has retired from active service. He has prepared a flora of the state for which he collected over 59,000 plants and examined in all 84,600 specimens.

Awards for research to members of the faculty of Wesleyan University for 1940–41 include Associate Professor Herbert E. Arnold, for a computation of statistical tables for testing the significance of experimental results; to Assistant Professor Jack Buel, for studies in the relative importance of specific response patterns and goal orientation in learning; to Professor Walter G. Cady, for studies in piezo-electricity; to Professor Burton H. Camp for a study of problems in mathematical statistics with particular reference to the theory of testing, and to Professor Frederick Slocum, for studies in stellar parallax determinations.

The Committee on Scientific Research of the American Medical Association has approved a grant to Dr. Oscar V. Batson, professor of anatomy in the Graduate School of Medicine of the University of Pennsylvania, for his quantitative studies of nystagmus.

The John and Mary R. Markle Foundation of New York has made a grant of $2,000 to the School of Medicine of Western Reserve University to support an investigation of intravenous bismuth injections by Drs. Torald Sollmann and Joseph Seifter, of the department of pharmacology.

Nature states that J. Davidson Pratt, general manager of the Association of British Chemical Manufacturers, has been appointed an additional deputy director-general for chemical research, experiment and development in the British Ministry of Supply, and Sir Frank E. Smith, director of instrument production, has, in addition, been appointed controller of telecommunications equipment.

Dr. Hermann de Jong, formerly director of the Physiological Institute of Amsterdam, is visiting the United States.

The Geologic Division of the Tennessee Valley Authority has been moved from Knoxville to Chattanooga. Major Edwin C. Eckel, chief geologist; Berlen C. Moneymaker, assistant chief geologist, and Robert A. Laurence, geologist, and their staffs will go to Chattanooga.

A course of lectures on recent advances in plant pathology is being given this summer at the Iowa State College, both by members of the faculty and by visiting lecturers. Visiting lecturers and their subjects are: Dr. William H. Stanley, associate member, the Rockefeller Institute for Medical Research, "Viruses," June 17 to 20; Dr. A. H. Reginald Buller, professor of botany at the University of Manitoba, "Fungi," June 24 to 27; Dr. George Keitt, professor of plant pathology at the University of Wisconsin, "Parasitism," July 1 and 2; Dr. C. M. Tucker, head of the department of botany at the University of Missouri, "Inheritance of Resistance to Disease," July 8 and 9; Dr. H. C. Murphy, pathologist, Bureau of Plant Industry, U. S. Department of Agriculture, "Resistance and Susceptibility in the Rusts and Smuts Parasitizing Oats," July 15. Members of the department of botany contributing to this course of lectures are: Professors I. E. Melhus, G. C. Kent and Walter Buchholtz.

The American Society of Ichthyologists and Herpetologists will hold its twenty-third annual meeting at the Royal Ontario Museum of Zoology, Toronto, on September 2, 3 and 4. The International Association of Game, Fish and Conservation Commissioners and the American Fisheries Society will meet in Toronto during the same week, at the Royal York Hotel, the former on September 2 and 3 and the latter on September 5 and 6.

The autumn meeting of the American Society of Mechanical Engineers will be held at Spokane, Washington, from September 3 to 6. A group of papers, of interest to mechanical, civil, electrical and mining engineers, is to be presented in sessions sponsored by the hydraulics, wood industries, heat transfer, fuels, power and management divisions of the society. The inspection trips, stressing wood industries, power developments, mineral industries and scenic wonders, are unique and of general as well as of technical interest. The program calls for technical sessions on Tuesday,

September 3, and Thursday, September 5, leaving Wednesday and Friday open for inspection trips. The trip to Grand Coulee Dam on Wednesday will be preceded by a general interest talk on the dam at the banquet on Tuesday evening. Friday is being left open for trips to the lumber mill at Lewiston, the mines of northern Idaho, the Columbia River Basin Project, or the scenic lakes of the region.

THE tenth annual meeting of the American Malacological Union was held at the Academy of Natural Sciences, Philadelphia, from June 17 to 21. The program included the presentation of a testimonial of appreciation to Norman W. Lermond, curator of the Knox Academy of Arts and Sciences, Thomaston, Maine, for his preliminary work in the organization of the society; and to Dr. Henry A. Pilsbry, curator of mollusks, Philadelphia Academy of Natural Sciences, its first president. In honoring Dr. Pilsbry on this occasion, the union is issuing a complete bibliography of his published works. This publication will be available for students of Mollusca by the end of August. Officers were elected as follows: *President,* Dr. Harald A. Rehder, U. S. National Museum; *Vice-president,* Frank Collins Baker, University of Illinois; *Corresponding Secretary,* Norman W. Lermond, Knox Academy of Arts and Sciences, Thomaston; *Financial Secretary,* Mrs. Harold R. Robertson, Buffalo Museum of Science. John Oughton, Royal Ontario Museum of Zoology, Toronto, was elected *Councillor at Large* to fill the vacancy occasioned by the resignation of Aurele La Rocque, of the National Museum of Canada. The next meeting will be held in the Knox Academy of Arts and Sciences, Thomaston, from August 5 to 8, 1941.

THE second annual exhibition of photographs of wild life under the auspices of the New York State Nature Association will be held at the Albany Institute of History and Art from October 30 to November 10. A first prize of \$20, a second prize of \$10 and a third prize of \$5 will be awarded to the pictures which best represent the spirit and beauty of living wild birds and animals photographed in their natural surroundings. The prize-winning photographs will become the property of the New York State Nature Association.

THE United States Civil Service Commission has announced an open competitive examination to fill the position of senior engineering aid (topographic) in the U. S. Geological Survey. The salary of the position is \$2,000 a year, less a retirement deduction of 3½ per cent. Except for the substitution of experience, applicants must have completed high-school study; and, in addition, must have had responsible civil engineering experience, partly on topographic field surveys. Certain engineering study in a college may be substituted for part of the experience. Applicants will not be given a written test, but will be rated on their qualifications, as shown in their applications, and on corroborative evidence. Applications will be rated as received at the commission's Washington office until December 31.

The Alumni Review of the University of North Carolina states that at a meeting of the trustees on June 7 a settlement proposed by its finance committee and agreed to by the executors and beneficiaries of the Flagler estate was approved. From this fund the university since 1917 has received annually the sum of \$75,000 to pay the Kenan professors. The settlement represents a generous interpretation by William R. Kenan, Jr., and his co-executor, Mr. Harris, and the chief beneficiaries of the will. The Kenan Fund was established in 1917 as a memorial to William R. Kenan, father of Mrs. Bingham, and her two uncles, Thomas S. Kenan and James Graham Kenan, all graduates of the university. The will provided that the university should be paid \$75,000 annually for twenty-one years, at the end of which period it should be paid an amount sufficient "at the rate of interest then current in North Carolina" to earn \$75,000 annually thereafter. Legal advisers to the executors suggested for transfer to the university securities valued at \$1,100,000 and yielding the amount of the annual income desired. Mr. Kenan, his co-executor, and Mrs. Graham Kenan and Mrs. Jessie Kenan Wise recognized, however, that funds reinvested in long-term securities by the university might not earn so high a yield. Accordingly, they agreed to a settlement of \$1,875,000 which in effect represented an additional gift by the estate, of which they are the chief beneficiaries, of \$775,000. The university in appreciation of their action added to the settlement sum an accumulated Kenan Fund reserve of \$182,000 together with \$43,000 from a pre-consolidation escheats fund to make the endowment of the Kenan Fund \$2,100,000.

DISCUSSION

THE NON-SPECIFICITY OF AMINO ACID CONFIGURATION IN MALIGNANT TISSUE HYDROLYSATES

THE development and application of the Krebs *d*-amino acid oxidase to the determination of total *d*-amino acid, described by us in these columns earlier this year,[1] provided a unique opportunity to subject

[1] F. Lipmann, O. K. Behrens, E. A. Kabat and D. Burk, SCIENCE, 91: 21, 1940.

to test both the broad claims and the expectations of Kögl and coworkers[2] in regard to cancer being characterized by proteins containing amino acids of partially unnatural (*d*-) configuration.[3] The uniformly low, randomly distributed values of 1 to 3 per cent. *d*-N of total-N found by us in the hydrolysates of a wide variety of appropriate and representative normal and tumor materials definitely eliminated the main claim of malignancy specificity with respect to total *d*-amino acid. The data obtained excluded a statistically significant difference between the malignant and normal tissue hydrolysate groups studied of greater than 0.5 per cent. in the observed average values of about 1.7 per cent. *d*-N of total-N in each group. Further search might, of course, reveal exceptional values in either group, but scarcely with appreciable bearing upon the generality of the conclusion reached.

In a recent note in SCIENCE Arnow and Opsahl[4] raise the point that *d*-glutamic acid, even if not total *d*-amino acid, might still be specifically characteristic of malignancy, providing the *d*-glutamic acid were of the same order of magnitude as the amino acids known to be racemized chemically during hydrolysis, and hence experimentally difficult to distinguish therefrom by the oxidase method. This suggestion, however, obviously introduces a ***double*** claim of malignancy specificity. For to assume that the malignant hydrolysate material contained some half of its *d*-amino acid-N specifically as *d*-glutamic acid-N—or much more than half as the literal 1.6 per cent. value proposed by Arnow and Opsahl would suggest—logically introduces the further implicit claim of a corresponding ***deficiency*** in the malignant material of half or more of that *d*-amino acid-N found in the normal material. Such a claim would, to say the least, be very difficult to maintain.

In regard to the experimental data of Arnow *et al.* presented in three other notes[5] held by them to be confirmatory of malignancy specificity, it may be pointed out that the purity of the glutamic acid hydrochloride samples claimed to have been isolated from tumor or normal tissue was not independently established by indispensable N or C and H (or ash) analyses, but only by melting point. The latter criterion, however, is of essentially no significance here, for we have observed that the melting point of *l*-glutamic acid hydrochloride is unaffected ($\pm 2°$ C) by NaCl impurity up to at least 50 per cent. With the Kögl procedure employed, we have frequently isolated materials of low rotation that proved upon analysis by the oxidase method to contain no *d*-glutamic acid, or far less than that indicated by the rotation; inorganic chloride and even small amounts of other occluded amino acids could be found which were difficult to eliminate upon one or two recrystallizations if these were carried out, as recommended by Kögl, with a weight loss of less than 10 per cent. It is obviously impossible, in the absence of adequate analytical control, to deduce the presence of *d*-glutamic acid merely from rotation values low for *l*-glutamic acid. The yields of materials isolated by Arnow *et al.*, where reported at all, were very scanty, and hence almost certainly stereochemically unrepresentative;[6] they were but one tenth to one hundredth the quantities of glutamic acid found in tissues upon complete isolation by the Foreman-type procedure,[6] by ourselves employing a considerably improved Kögl procedure involving vigorous stirring during crystallization or by isotopic analysis.[7] It is difficult to see that the data of Arnow *et al.*[5] have much if any bearing on the question of malignancy specificity at hand.

As to the possibility that *d*-glutamic acid occurs specifically in malignant material as a ***small*** fraction of the total *d*-amino acid, there is now developing abundant evidence that, providing appropriate methods of isolation or analysis are employed, *d*-glutamic acid is to be found in hydrolysates of both normal and tumor material, and to about the same small extent in each. As indicated in our earlier communication on the oxidase method,[1] no attempt was made to determine individual *d*-amino acids, in particular *d*-glutamic acid, in hydrolysates, except *d*-amino acids intentionally added

[2] F. Kögl and H. Erxleben, *Zeits. physiol. Chem.*, (*a*) 258: 57, 1939; (*b*) (with A. M. Akkerman) 261: 141, 1939; (*c*) 261: 154, 1939; (*d*) (with H. Herken) 263: 107, 1939; (*e*) 264: 108, 1940.

[3] In any consideration of the Kögl claim it is important to bear in mind that this claim was made with respect to a variety of amino acids, including not only glutamic acid, the most prominent and easiest amino acid to isolate in this connection, but also at least leucine, lysine, valine, hydroxyproline, hydroxyglutamic acid and arginine, and even possibly to some extent those amino acids known to be racemized in part by the acid hydrolysis employed but provisionally excluded by Kögl from closer quantitative analysis because of the experimental finesse required. In his early papers Kögl laid frequent emphasis on the presence in malignant tumors of what he called "partially racemic protein," of "a derangement of stereochemical specificity possibly propagated from glutamic acid to other amino acids," or "*d*-forms of still other amino acids (even indispensable ones) that might be found in residual mother liquors." In the opening sentence of a recent paper[2d] Kögl has continued to stress his broad claim, adding a note of reproof, however, for the exclusive attention being paid by others to glutamic acid: "The discovery of partially racemic amino acids in tumor protein has quite understandably induced various workers to check our findings, but with restriction, above all, to the extreme case of glutamic acid."

[4] L. E. Arnow and J. C. Opsahl, SCIENCE, 91: 431, 1940.

[5] L. E. Arnow and J. C. Opsahl, (*a*) SCIENCE, 90: 257, 1939; (*b*) (with C. J. Watson), *Proc. Soc. Exp. Biol. Med.*, 43: 766, 1940; (*c*) (with W. G. Clark) *Proc. Soc. Exp. Biol. Med.*, 43: 767, 1940.

[6] A. C. Chibnall, M. W. Rees, E. F. Williams and E. Boyland, (*a*) *Nature*, 145: 311, 1940; (*b*) *Biochem. Jour.*, 34: 285, 1940.

[7] S. Graff, D. Rittenberg and G. L. Foster, *Jour. Biol. Chem.*, 133: 745, 1940.

as controls, but reference was made to forthcoming determinations based upon isotopic analysis with deuterium, a second method of *d*-amino acid analysis not involving the incomplete or unrepresentative isolation procedures employed by Kögl or Arnow. Details must still await full publication, but we may briefly report here that the samples B and D of human normal liver and liver carcinoma earlier found[1] to contain 2.4 and 1.7 per cent. total *d*-amino acid-N of total-N both showed upon analysis with deuterium the definite presence of small, approximately equal quantities of *d*-glutamic acid of the order of several tenths of a per cent. of the original dry weight of tissue. These results agree very well in order of magnitude with the bulk of values reported by others for normal or malignant tissues by various methods, and in particular by Chibnall *et al.*[6] for fairly complete isolation,[8] by Graff, Rittenberg and Foster[7] from isotopic nitrogen analyses, and from expectation based upon glutamic acid racemization[9] now to be reported.

Probably the most direct, simple and conclusive evidence against *d*-glutamic acid malignancy specificity is the fact now definitely established that—contrary to earlier indications (2a, p. 73; 6, p. 294)—*l*-glutamic acid itself is racemized in hot hydrochloric acid. The rates under varying usual conditions are small but readily measurable. They are of an order needed to account for essentially all the isolated or analyzed quantities of *d*-glutamic acid reported in hydrolysates of protein, normal or tumor tissue by Kögl *et al.*,[2a-d] others since,[5, 6, 7, 10] and ourselves above. In our experiments *l*-glutamic acid, when refluxed under the same conditions as employed in our earlier protein and tissue hydrolyses,[1] was converted to *d,l*-glutamic acid at an average, nearly constant rate of 0.3 per cent. per hour up to at least 50 per cent racemization. The formation of *d*-glutamic acid, measured by the decreasing specific rotation of samples removed from time to time, was further confirmed and completely established by both *d*-amino acid oxidase analysis and isolation of analytically pure partially racemic glutamic acid hydrochloride in good yield. Kögl *et al.*[2a-d] have reported 29 isolations of glutamic acid from human, rabbit and rat tumors, with an average yield of 0.4 per cent. *d*-glutamic acid of tissue dry weight. This, assuming an average of about 10 per cent. total glutamic acid in tissue dry weights,[6, 7] is an average of about 4 per cent. racemization, or of the same rough order that might be expected from simple racemization of *l*-glutamic acid during hydrolysis. Any close comparison here would obviously call for a detailed consideration of actual concentrations of reactants during hydrolysis, isolation yields, the effect, if any, of combination of glutamic acid in protein linkages, etc. Two of Kögl's tumors yielded no glutamic acid, whereas only four gave more than 1 per cent. *d*-glutamic acid of dry weight, two of these attaining 3.7 and 4.2 per cent. These latter two values, obtained by Kögl early in his work, can well be regarded, even in the light of Kögl's own work, as definitely not typical or characteristic of tumors. It would seem unprofitable or futile, therefore, to make much point concerning them, as do Arnow and Opsahl in their last note[9] on the subject at hand, in an attempt to maintain malignancy specificity on the basis of these two atypical cases, now that virtually all other cases, to the number of several score, can be readily understood on a basis of simple glutamic acid racemization during hydrolysis.

CONCLUSION

The view of malignancy specificity proposed by Kögl and supported by Arnow *et al.*, that cancers but not normal tissues or proteins are composed of partly unnatural (*d*-)amino acids, is clearly no longer tenable, both as regards *d*-glutamic acid as well as total *d*-amino acid.[11]

We again take pleasure in expressing our appreciation and indebtedness to Professor Vincent du Vigneaud for his continued counsel during these investigations.

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DIOXYMALEIC ACID OXIDASE

WE have been much interested in the dioxymaleic acid oxidase of Banga and Szent-Györgyi[1, 2] and in

[8] Concerning the unsuccessful isolations of *d*-glutamic acid from normal tissues by Kögl, Arnow and others, or from tumors by still others, further reference may be made to the detailed discussion and explanation by Chibnall *et al.*[6]

[9] L. E. Arnow and J. C. Opsahl, *Jour. Biol. Chem.*, 133: 765, 1940; J. M. Johnson, *Jour. Biol. Chem.*, 134: 459, 1940.

[10] (*a*) J. White and F. R. White, *Jour. Biol. Chem.*, 130: 435, 1939; (*b*) C. Dittmar, *Zeits. f. Krebsforsch.*, 49: 441, 1939; J. M. Johnson, *Jour. Biol. Chem.*, 132: 781, 1940; H. Ottawa, *Zeits. f. Krebsforsch.*, 49: 677, 1939.

[11] Added to galley proof: In an article just come to hand by F. Kögl, H. Herken and H. Erxleben (*Zeits. physiol. Chem.*, 264: 220, 1940), the applicability of the *d*-amino acid oxidase method employed by us in our earlier communication (footnote 1) is questioned in several respects. In our full publication, now in preparation, we are presenting a complete description of the method and of the data obtained, which will demonstrate in detail the validity of the earlier results and conclusions reached by us.

[1] I. Banga and A. Szent-Györgyi, *Zeits. physiol. Chem.*, 255: 57, 1938.

[2] I. Banga, E. Philippot and A. Szent-Györgyi, *Nature*, 142: 874, 1938.

the evidence of Theorell and Swedin[3,4] which indicates that this new enzyme is really identical with peroxidase. We find that in the presence of fresh alcoholic dioxymaleic acid, acetate buffer of pH 4.5, partially purified horse-radish peroxidase and hydrogen peroxide, methyl red/o-carboxy-benzene-azodimethylaniline/ is rapidly oxidized and decolorized. In the absence of dioxymaleic acid the oxidation is slow. Moreover, peroxide need not be added, provided that the solution of dioxymaleic acid, methyl red, acetate buffer and peroxidase is shaken with air. In nitrogen there is no decolorization.

We find that peroxidase, as shown by testing with guaiacol and hydrogen peroxide, is rather rapidly inactivated by buffered dioxymaleic acid, and that this inactivation is retarded by aeration. Inactivated peroxidase is less effective in oxidizing dioxymaleic acid and in decolorizing methyl red than is active peroxidase.

Old alcoholic solutions of dioxymaleic acid give different results from fresh solutions. Here, a mixture of dioxymaleic acid, acetate buffer and peroxidase decolorizes methyl red even in nitrogen, while aeration largely prevents decolorization.

Our explanation of the rapid bleaching of methyl red in the presence of fresh dioxymaleic acid, peroxidase and hydrogen peroxide is that the peroxidase and peroxide convert the dioxymaleic acid into diketosuccinic acid and that the methyl red is oxidized by a coupled reaction. If one shakes dioxymaleic acid solutions in air, it is not necessary to add peroxide, since dioxymaleic acid is spontaneously oxidized and forms peroxide. This is in agreement with the evidence of Theorell and Swedin. Old solutions of dioxymaleic acid already contain peroxide, so that they require no shaking with air. We are not able to tell why shaking with air should retard the decolorization of methyl red in the presence of old dioxymaleic acid.

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Alexander L. Dounce
Cornell University

THE EFFECT OF GROWTH SUBSTANCES ON THE ROOTING OF BLUEBERRY CUTTINGS

Three experiments have been conducted to study the effect of growth substance on the rooting of summer cuttings of blueberries, *V. corymbosum.* These experiments were carried on in 1937 and 1938, using in all over 2,500 cuttings from eleven different varieties of blueberries. Indole-3-acetic acid and indole-3-proprionic acid were each used dry and in solutions of 5 and 10 mg per liter. Phenyl acetic acid was used only in solutions of 10, 25 and 50 mg per liter. Auxilin was used at the recommended concentration No. 3. Phenyl acetic acid was the only one of the growth substances used which significantly increased the percentage of rooting and the greatest increase was at the medium concentration, 25 mg per liter. The results obtained with auxilin were very poor at the concentration used. The results of the above experiments are in agreement with those of Stanley Johnston's investigations independently conducted at the same time and reported in *Michigan Station Quarterly Bulletin,* 21: 255–8, 1939. From the results thus far obtained it does not seem advisable to recommend the use of growth substance for the rooting of blueberrry cuttings.

F. B. Chandler
I. C. Mason
University of Maine

THE IDENTITY OF THE TREE "ANNEDDA"

From your review of "The Englishman's Food"[1] it appears that the authors think the tree (called by the Indians "Annedda") which cured Jacques Cartier's men of scurvy when they were wintering at Stadacona in the winter of 1535–36, was *Sassafras officinale.* The sassafras does not grow anywhere in the Province of Quebec. Its only station in Canada is a relatively narrow strip in southern Ontario and there is no reason to suppose that it ever ranged farther north.

In support of the sassafras as against an evergreen, the authors state, according to the reviewer, that Cartier's notes particularly refer to the fact that the Indians had to wait for the leaves to appear in the spring. Perhaps authority is given for this statement, but no such passage occurs in Biggar's edition of "The Voyages of Jacques Cartier." Furthermore, Cartier says[2] that it was while he was walking on the ice that the Indians told him of the tree which would cure the sickness and two squaws went with him to gather some of it. Nine or ten branches were brought back, and Cartier adds: "They showed us how to grind the bark and leaves and to boil the whole in water."

The identity of the tree Annedda has been much disputed, but from considerations not necessary to discuss here, it seems likely that it was the hemlock, *Tsuga canadensis.*

Charles Macnamara
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THE KIT FOX

In the summer of 1911, T. A. Rocklund and I were camped for one month on the Pennell Ranch in Wallace County, Kansas, adjoining the George A. Allman

[3] H. Theorell and B. Swedin, *Naturwissenschaften,* 27: 95, 1939.
[4] B. Swedin and H. Theorell, *Nature,* 145: 71, 1940.

[1] Science, 91: 217, 1940.
[2] H. P. Biggar, "The Voyages of Jacques Cartier," p. 212–215. Ottawa: The King's Printer, 1924.

Ranch. Mr. Allman was an early settler in the West, had been a government guide, and had shipped tons of fossils to the Smithsonian Institution. He was a very observing man and spent hours recounting to me conditions of wild-life in the early West.

He told me that it was the general custom of the early cattlemen to place poison at all the undevoured buffalo carcasses to destroy the wolves. It was his observation that the little swift foxes were always the first to take the poison. He stated that the gray wolf had not been seen in Kansas since 1879.

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SCIENTIFIC BOOKS

THE SOCIAL ORDER

Human Nature and the Social Order. By E. L. THORNDIKE. xx + 1019 pp. New York: The Macmillan Company. 1940.

THE desirability of bringing the methods and results of the natural sciences to bear on important social problems, though often emphasized, is seldom acted on by the natural scientist in any thoroughgoing way. It takes time and courage to do so. Thorndike's new book shows courage and proves that its author has devoted much time to the study of social problems. Some of them, touching on his own special fields of psychology and education, he has attacked in original investigations. The general aim of the present book is to show how what we already know of biological psychology can be applied to social problems, and how a natural-science background enables us at least to plan an attack upon these problems. Courage is needed when existing knowledge is insufficient to furnish a direct answer. The author often ventures a well-considered judgment even when admittedly unable to offer a fully scientific solution.

The book consists of two main parts, the first, presenting certain accepted findings of psychology (and of genetics) which are especially pertinent to social problems, and the second, considering these problems in some detail. The first part, running to 400 pages, will be welcomed as a systematic account of Thorndike's main contributions to psychology. It treats of the native equipment of man, of the laws of learning, of abilities and motives and their measurement, of individual differences in ability and motivation, and of a projected science of human values.

An ability is best defined in terms of results accomplished under given conditions. So defined, abilities are very specific and particularized. The ability to add 6 and 9 is not completely identical with the ability to add 8 and 7, for an individual may show full mastery of one of these sums and still be quite shaky as regards the other. Instead then of assuming a few great "faculties of the mind," Thorndike starts with these numerous specific abilities and inquires how they can legitimately be grouped. The best empirical principle is to place together abilities that are found to exist together in the same individuals, as indicated by the correlation method.

In any sort of ability there are several variables that can be measured, such as speed, quality of work, difficulty of tasks accomplished and width or extent of the ability. In arithmetic, for instance, one person is quicker than another, one is more accurate than another, one can handle examples of greater difficulty, and one can handle a greater variety of easy examples or of examples at any level of difficulty. Since these variables can be measured, ability can be measured, though not always easily or simply.

Motives, or "wants," are treated in the same general way as abilities. In neither case is it possible at present to define and distinguish most of them in terms of the cerebral and other intraorganic operations by which external results are accomplished. Wants are necessarily defined in terms of results wanted under given conditions. We have to start our science of wants by recognizing a great multiplicity of particular wants and we find it difficult to go behind this multiplicity and discover any adequate system of fundamental or inclusive wants. Relatively few concrete human desires belong strictly under the traditional needs for self-preservation and propagation of the species. Many wants have certainly been acquired by the individual in dealing with his physical and social environment. The relative strength of various wants can be estimated from the individual's use of his leisure time and from his expenditure of his money earnings (representing his working time). On the basis of information collected on these two points, the author estimates (p. 135) that

> the 16 hours of the waking day of adults in the United States are spent roughly as follows:
>
> 25 per cent. for subsistence and perpetuation.
> 2 " " to avoid or reduce sensory pain.
> 7 " " for security.
> 8 " " " the welfare of others.
> 30 " " " entertainment.
> 10 " " " companionship and affection.
> 10 " " " approval.
> 4 " " " intellectual activity.
> 2 " " " dominance over others.
> 2 " " " other wants.

But are psychological variables such as ability and

the strength of a motive susceptible of true measurement? Some critics deny it, but Thorndike has consistently stood for measurability. He would insist "that any want or satisfaction which exists at all exists in some amount and is therefore measurable, how exactly and how commensurably with others, we can not tell till we have tried" (p. 152). Measurement of wants has one advantage over measurement of abilities, because a true zero can easily be determined. Indifference, the condition of neither seeking nor avoiding a certain result, is a true zero of want intensity. If one positive want can be fairly estimated to lie twice as far above zero as another want, we have a start toward measurement. With suitable precautions, the time the individual devotes to securing a satisfaction (in comparison with other satisfactions), the amount of hardship he will undergo or the amount of money he will spend to secure a satisfaction, can be used as indirect measures of the strength of a want. In principle, then, a quantitative science of motivation is possible.

In proportion as wants are known and measured, an empirical science of values, a naturalistic ethics, becomes possible. Values are relative to wants. The value of any human action depends on its consequences, so far as they satisfy or annoy human beings or other sentient beings. Thus, the value of an act or of a thing is a natural fact—not transcendental nor absolute—and this fact is, in principle, observable and measurable. Measurement is difficult, to be sure, since different wants of the individual, and the wants of all the individuals affected by an act, must be considered and properly weighted before the most correct valuation is reached. The wants of different individuals require very unequal weights in some instances. The want of a creative artist to produce has much more far-reaching consequences in the lives of men, and therefore deserves much more weight, than the want of one ordinary man for a good dinner.

The existence of large differences between men both in wants and in abilities is a fundamental fact for social psychology. The actual distribution of ability in the population and the correlation of one trait with another are among the basic facts, even without regard to the ultimate causes of these variations. The causes also are of great importance, especially when the future welfare of mankind receives the serious consideration that it deserves. The author is convinced that hereditary factors, the genes, are responsible for a large share of the variation in intelligence that we find in a community—perhaps 80 per cent. of it, the remainder of the variation being mostly attributable to differences of training. Certainly ability of a high order is impossible without a favorable combination of genes. Hence, the great importance of eugenics for conserving and increasing human resources. "Improvement of the human genes, though much slower than some enthusiasts have represented it, is the surest means of fostering the good life; it operates at the source by producing better people. It also produces indirectly better customs and institutions" (p. 453).

Improvement of the human stock by eugenic measures is bound to be slow because the genic determination of important human traits is highly complex. But the average can be raised by measures that prevent propagation of the most unfit or favor that of the most fit. There is no prospect of raising the upper limit and producing a race of supermen, but what is most essential is to insure a plentiful supply of men and women in the upper brackets of ability, character and personality, since it is on these high-class individuals that the whole population depends for all kinds of progress and even for operating our civilization at its present level. A scientific program of eugenics must avoid any narrow definition of fit and unfit, and it demands much preliminary research into the correlations of desirable traits. But even to-day it is quite feasible by sane and moderate measures to combat the visible tendency of civilization toward elimination of the most promising strains. One measure would consist in "marriage allowances and allowances for children to men of very high intelligence and achievement during the period from 21 to 30," when most of them now receive very low wages, and another measure would be the provision of 10,000 college scholarships for children of proved ability. This last measure "would probably cause a considerable number of promising babies to be born, by removing the fear of the very definite and large costs of college education" (pp. 458, 459).

The psychology of learning has been one of Thorndike's favorite research fields since the beginning of his career. His main findings are incorporated in his celebrated "law of effect" or, more recently, in his concept of the "confirming reaction." Mere repetition of an act establishes it to a slight degree, but the immediate consequences of the act to the doer of it are the most important factor in learning. If the immediate consequences are satisfying, they release a confirming reaction in the brain. The confirming reaction reinforces the act in one way or another, causing it to be continued, immediately repeated or, if this is impossible, causing it to be "stamped in," impressed on the nervous system so as to be more readily executed on a later occasion. If the immediate consequences of an act are annoying to the doer, we might expect something opposite to the confirming reaction. We do find a cessation of the act and a shifting to some other act, but we get no evidence of a direct process of "stamping out." There is no inhibitory reaction commensurate with the confirming reaction. Reward, then, is a more powerful instrument than punishment. Such

is the outcome of recent experiments by Thorndike and others. Formerly, the theory of learning treated the two as on a par.

The traditional doctrine of common sense and of all the sciences of man was that rewards and punishments were the positive and negative halves of one same scale or gradient, closely alike in potency. . . . This doctrine is false in certain important respects. . . . Except when and as it causes the person to shift to the right behavior and receive a reward therefor, the punishment has no beneficial effect comparable to the strengthening by a reward. . . . Psychology recommends that punishment be used only when and as it can be proved to be effective. . . . In the case of government, psychology emphasizes the importance of making a community attractive to the able and good rather than unpleasant for those who are incompetent and vicious. . . . The law is rightly skeptical about restrictive and punitive activities, but has not yet progressed far with alternatives. . . . Its voice is threatening and its acts are punitive. . . . Business, in contrast to government, has operated largely by rewards. . . . In general, the maxim "Reward good behavior" is as nearly a golden rule as any that psychology has to offer human society (pp. 199–207).

Labor is often conceived as pure punishment or annoyance, its only rewards lying in the pay envelope. Such a view overlooks the genuine rewards that spring directly from bodily and mental activity, from achievement and mastery, from companionship and participation, from the approval of one's fellows and even from loyal submission to the right sort of boss. Management, then, should take account of the whole man and not simply of the abstract "economic man," and it should see the man in his total life situation rather than simply while on the job.

Many other problems of industry, government and human welfare are approached in the author's characteristically candid and freethinking spirit, and the net effect is to leave the reader with a hope of ultimate solution. Perhaps the main contention of the whole book is that science is the best guide for human life. "The acceptance of impartial scientific truth as the guide in life is certainly a safe and sane policy, and can be a very idealistic one" (p. 390). "The social sciences are still weak and insecure; the doctors often disagree. Some can be found to support fantastic schemes. But it is surely better on the average to take their medicine than that of ignoramuses. The scientific method is dependable. Proposed treatments to cure political, economic, or other social ailments should be studied as far as possible by the impartial methods of science" (p. 958). Governments are constantly making experiments which would have scientific value if provision were made for tracing the consequences of any given legislative or administrative measure. "Our consideration of human nature in relation to human welfare has brought forth no panacea, and promises no miracles of any description. But it has shown that man has the possibility of almost complete control of his fate, and that if he fails it will be by the ignorance or folly of men" (p. 957). Even now we can see certain dependable guiding principles, looking toward the improving of the population and the provision of suitable education and opportunity for those who are able to render public service. Able and good men must not rest content with inferior positions in the world's councils. They must acquire power so as to make their ability and good-will count heavily in social affairs.

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SOCIETIES AND MEETINGS

THE CELEBRATION OF THE OHIO ACADEMY OF SCIENCE

JUST fifty years ago, some fifty-four serious-minded, forward-looking seekers after truth (scientists) gathered in a small room of the old Central High School, in Columbus, Ohio, for the avowed purpose of forming an organization that would be "an inspiration and a stimulus to original research and investigation." And so the infant *Ohio Academy of Science* was born and during the fifty years of its existence has grown in size and usefulness, fully justifying, we believe, the prophecy made by Professor William R. Lazenby, the first secretary, namely, that "Once organized, I am sure the Ohio Academy would be a signal and all-inspiring success, and could scarcely fail to secure an honored position among the scientific organizations of our country." In fact, the academy had attained such influence and importance that four years ago, in anticipation of the approaching fiftieth anniversary, and at the suggestion of Dr. F. C. Waite, of Western Reserve University, it resolved: "That the incoming President appoint a preliminary committee of five on plans for the celebration and that this committee be charged to make a definite report on plans at the meeting next year."

Accordingly, a committee of which the writer was made chairman was appointed and did make a report at the annual meeting in 1937, the report heartily approving the idea of a celebration and suggesting that "it be done in a distinctive and comprehensive manner that will not only commemorate what has been accomplished in the past fifty years, but will also stimulate yet greater endeavor in the future."

The plan was unanimously approved and action taken at once to carry it out. It was enthusiastically agreed that our late honored and highly esteemed

co-worker, Dean George F. Arps, head of the Graduate School at the Ohio State University, was the very man for director, and he was unanimously chosen and accepted. Dean Arps, in his fine, enthusiastic way, laid out a most ambitious program and was busily engaged in setting up the machinery to carry it out when the "grim reaper" took him from us. For a brief period, confusion and discouragement prevailed, but, as so often happens under such circumstances, the hero emerges and it was so in this case! We found in our presidents the leadership we so sorely needed: first, President Claude E. O'Neal, of Ohio Wesleyan University, and later President William Lloyd Evans, Ohio State University. They stepped nobly into the breach and carried on to what we feel was a triumphant success.

The celebration was set for May 9, 10 and 11, at Columbus, the birthplace of the academy. The attendance was the largest in the history of the academy, and the program was full and rich, quite a number of speakers of national not to say international fame were on the program. The program began with a general meeting on Thursday evening at which His Excellency, the Governor of Ohio, John W. Bricker, spoke, also President Bevis, of Ohio State University; but the event of the evening was the address of Dr. Harvey Fletcher, of the research staff of the Bell Telephone Company, on "Sound Patterns."

Friday morning was given over to three concurrent symposia of great interest that were discussed by eminent authorities: (1) A symposium on "Hearing," discussed by R. H. Stetson and E. A. Culler; (2) a symposium on the "Basic Factors in Conservation," discussed by Guy W. Conrey, Frank J. Wright and E. N. Transeau; (3) a symposium on "Radiation and the Cancer Problem," discussed by E. U. Condon, Isadore Lampe and Francis Carter Wood. The afternoon was given over to one general meeting with three most interesting addresses: (1) Dr. R. W. Chaney, of the University of California, discussed "Forests on a Changing Earth"; (2) Dr. John L. Rich, University of Cincinnati, spoke on "Application of Airplane Photography to Geographic Studies," and (3) Dr. Charles F. Kettering, of the General Motors Corporation, spoke on "Industry and Science."

On Friday evening occurred the annual dinner and business meeting, which, as usual, was the climax of the general meetings. Two addresses were made on this occasion, one by Dr. James P. Porter, Ohio University, on "The History of the Ohio Academy of Science," and the other, a masterly presidential address, by Dr. William Lloyd Evans on "A Present-day Examination of the Postulates of John Dalton." The annual election of officers occurred at this meeting; the results are given below.

On Saturday morning the program consisted again of three concurrent symposia and in addition a botanical program. The first symposium was on "The Nervous System" and was discussed by Dr. Herbert S. Gasser, of the Rockefeller Institute, New York, by Dr. R. W. Gerard, of the University of Chicago, and by Dr. John F. Fulton, of Yale University. The second symposium was a Chemical-Physical Program and was discussed by Dr. Edward Mack, of the Battelle Memorial Institute, Dr. M. S. Newman, Ohio State University, and Dr. H. S. Booth, Western Reserve University. The third group discussed a "Genetics-Speciation Program," the leaders in the discussion being Dr. Laurence H. Snyder, of Ohio State University, Dr. Warren P. Spencer, College of Wooster, and Dr. David C. Rife, Ohio State University. In the botanical program four papers were presented: one by Dr. A. E. Waller, of the Ohio State University on "John H. Schaffner—a Résumé"; one by Dr. J. H. Gourley, Ohio Agricultural Experiment Station on "The Changing Point of View in Horticultural Research," one by Dr. Paul B. Sears, Oberlin College, on "Post-glacial Vegetation in the Ohio-Erie Region," and the fourth by Dr. E. Lucy Braun, University of Cincinnati, on "The Differentiation of the Deciduous Forests of the Eastern United States."

So much for the formal program—excellent in every detail. The meeting, however, was characterized by at least three other notable features: (1) The number and excellence of the exhibits; these were assembled and arranged by the Exhibits Committee under the leadership of Dr. Glenn W. Blaydes, of Ohio State University. Note that at the date of the printing of the program he had collected ninety-nine centralized exhibits and arranged for forty fixed exhibits; others probably came in later. The exhibits would have done credit to the American Association for the Advancement of Science; (2) The second notable step forward was the organization of a *Junior Academy Section,* under the inspirational leadership of Dr. Otis W. Caldwell, of the American Association for the Advancement of Science, and the persistent, intelligent, skillful, enthusiastic leadership of Dr. Charles W. Jarvis, of Ohio Wesleyan University, ably assisted by Mr. Orville Linebrink, of the Columbus high schools. Do you know these juniors came from many high schools in the state, and "believe it or not," they entered some sixty-seven or more papers, exhibits, demonstrations, etc., in the contest, any one of which would have done credit to their elders. If we may, without seeming to discriminate, we would like to refer to two of these: one, the "Dissecting of a Pig Embryo" put on by four girls from the Walnut Hills High School, in Cincinnati, namely, Katherine Baude, Margie Richmond, Anne Jane Theiler and Helen Weide-

man, with Miss Etta Elberg as sponsor; the other was entitled "Experiments Using Dry Ice," put on by two boys from the New Philadelphia High School, George Robb and Glenn Curtis, Miss Leila E. Helmick, sponsor. Both demonstrations were put on in a masterly manner, doing great credit to the pupils, the teachers and the schools from which they came. (3) The third notable advance, while not so spectacular, is very real and is found in the change in the form of government of the academy from the general to the council form of government, as provided in the revised constitution—somewhat after the manner of the American Association for the Advancement of Science.

As a result of a rather intensive campaign under the slogan "One Thousand Members in 1940" and under the leadership of Dr. C. W. Jarvis, the membership list of the academy was increased by 127 new members at the annual meeting, and still they come, and by three restorations so that we are well on the way toward 700, to say nothing of the Juniors.

The annual election resulted in the choice of the following as the officers for the ensuing year: *President,* Stephen R. Williams, Miami University; *Secretary,* William H. Alexander, Normandie Hotel, Columbus; *Treasurer,* Edward S. Thomas, Ohio State University.

WILLIAM H. ALEXANDER,
Secretary

THE ILLINOIS STATE ACADEMY OF SCIENCE

THE Illinois State Academy of Science met for its thirty-third annual meeting at Galesburg, Illinois, May 3 and 4. The meeting was well attended and facilities provided by Knox College and by the City of Galesburg were unusually well planned. All meetings progressed nicely on schedule, adding much to the pleasure and interest. The main features of the meeting on Friday were the general session in the morning, the special sections meeting in the afternoon, the Junior Academy exhibit and judging, banquets of the Junior and Senior Academies in the evening, followed by a joint lecture for both groups after the banquets. Awards for prize-winning exhibits in the Junior Academy were made following this lecture.

At the morning session three lectures were given: Dr. E. I. Fernald, professor of botany at Rockford College, and the retiring president of the academy, spoke on "Michael S. Bebb, Illinois Botanist and Letter-Writer." The lecture was illustrated with slides. Two other lectures, "Science and Common Life," by Anton J. Carlson, of the University of Chicago, and "Looking through Great Telescopes," by Oliver J. Lee, Dearborn Observatory, completed the morning program.

Arrangements were provided by the college for a joint luncheon, special tables and rooms being available for groups that desired this. The geologists met together and the Illinois branch of the American Association of Physics Teachers also. This latter group sponsored the physics section program and appointed a committee to organize an engineering and applied science section for the next annual meeting.

Altogether some 120 papers were presented at the section meetings in the afternoon. The chairmen elected to take charge of the respective sections for the ensuing year were:

Agriculture: Mr. C. H. Oathout, Western Illinois State Teachers College.
Anthropology: Mr. Floyd Barloga, 1423 N. Glenn Oak, Peoria.
Botany: Mr. Paul Voth, Department of Botany, University of Chicago.
Chemistry: Mr. George H. Reed, Knox College.
Geography: Dr. Arthur B. Cozzens, University of Illinois.
Geology: Mr. J. Marvin Weller, State Geological Survey.
Physics: Ph. A. Constantinides, Wright Junior College.
Psychology and Education: O. Irving Jacobsen, Shurtleff College.
Zoölogy: Dr. W. V. Balduf, Department of Entomology, University of Illinois.

It was voted to accept the invitation of a cooperating group headed by the Museum of Science and Industry, Chicago, to hold the next annual meeting there on May 1, 2 and 3, 1941.

The evening banquet was well attended and adjourned to enjoy an exciting demonstration lecture given by Dr. J. O. Perrine, of the American Telephone and Telegraph Company, who led the discussion with Pedro the Voder. The meeting was a joint one with the Junior Academy, and both groups are indebted to Dr. Perrine and the company he represented for the lecture. After the lecture awards were presented to the winners in the competition between the science groups of the Junior Academy and the American Association for the Advancement of Science. It was an enthusiastic occasion and demonstrated quite clearly the vitality of the academy work in the state of Illinois.

On Saturday morning the Burlington Railroad took a group of over a hundred Junior and Senior Academy members on a special train through their yards and tie-treatment plant in the vicinity of Galesburg. Three other science pilgrimages to points of interest in that part of the state were planned and carried out by the anthropologists, geologists and biologists. All trips were reported successful and well attended. The academy meeting was attended by over six hundred. It is anticipated that even a larger group will be in attendance next year when the meeting will be in Chicago.

Officers for the coming year are:

President: V. O. Graham, 4028 Grace Street, Chicago.
First Vice-president: T. H. Frison, State Natural History Survey, Urbana.
Second Vice-president: C. R. Moulton, Museum of Science and Industry, Chicago.
Secretary: R. F. Paton, University of Illinois.
Treasurer: John Voss, Manual Training High School, Peoria.
Librarian: Thorne Deuel, State Museum, Springfield.
Editor: Grace Needham Oliver, State Geological Survey, Urbana.
Junior Academy Representative: Miss Audry Hill, Chester High School, Chester.

Winners of the American Association for the Advancement of Science academy grants in aid for research for 1940–41 were:

Dr. F. O. Green, Department of Chemistry, Greenville College.
Dr. C. L. Furrow, Department of Zoology, Knox College.
Dr. F. R. Cagle, Southern Illinois State Normal University.

R. F. PATON,
Secretary

SPECIAL ARTICLES

DIMINISHING RESPONSE OF THE SKIN TO FREQUENTLY REPEATED REINFECTION WITH INVASIVE BACTERIA

IN experiments concerned with the mode of action of sulfanilamide we wished to make observations on the difference in the response to the drug by a primary infection as compared to the response found once an infection had been established. In devising the experiments we felt that injecting four or five small doses of the bacteria in different parts of the skin, at intervals of a few hours, would give more information than injecting a single large dose. As a preliminary step leading to this study, rabbits were injected intradermally, following this procedure but without sulfanilamide being administered. The main purpose of this preliminary note is to describe a phenomenon which was encountered constantly in the course of these studies. Details of the experiments will be published in the *Yale Journal of Biology and Medicine.*

Two groups of bacteria were studied, (*a*) two strains of *Streptococcus hemolyticus* and two strains of *Staphylococcus aureus,* which were invasive bacteria and produced large amounts of spreading factor, and (*b*) two strains of *E. coli* and one of *S. anolium,* which were not invasive and did not produce spreading factor. Broth cultures of exactly the same age were employed for the injections in each rabbit. In general, the amount of bacterial culture administered in each injection was that which when injected for the first time into rabbits would induce lesions measuring from 10 to 20 sq cm after 24 hours. For instance, when streptococcus was employed 0.1 cc of 18-hour broth culture was usually injected; when *E. coli* was employed 0.5 cc of culture were injected.

It was found that when the invasive bacteria were injected in four or five identical doses within a period of from one to twenty-four hours, using a different part of the skin for each injection, the resulting lesions, measured twenty-four hours after injection, showed marked differences both in severity and in the area through which the infection had spread; the second lesion being smaller and less severe than the first; the third smaller than the second, and the fourth and fifth, when present, smaller and much less severe than all the lesions resulting from earlier injections. These results were not affected by the site injected. The phenomenon may distinctly manifest inself as early as one hour after the first injection. Frequently the area of the lesion resulting from the last injection was as much as fifteen times smaller than that resulting from the first; indeed, oftentimes such lesion was no more than a pimple.

This phenomenon of the diminishing skin response was completely absent in infections caused by *E. coli* and *S. anolium.* However, when larger doses of these non-invasive bacteria were given so as to induce a more severe first lesion, a slight decrease in the response to the following injections was sometimes observed; and if the same amounts of bacteria were injected together with spreading factor either from streptococcus or from testicle extracts so that very large first lesions were induced, then the phenomenon was clearly present.

Antibodies were not found in the serum by the usual serological tests, and blood counts made at different intervals showed no unusual or marked variations.

These findings led us to make identical experiments with heat-killed bacteria and with bacterial filtrates. The results were as follows:

The phenomenon was practically never present when the bacteria, either invasive or non-invasive, were killed by heat. In testing the filtrates India ink was used as an indicator of the area through which each inoculum spread. Filtrates of non-invasive organisms failed to spread at all, and the area of spread of all inocula was the same. Streptococcus filtrates, endowed with a pronounced spreading power, seemed to elicit the phenomenon in some instances, and not in a very marked degree.

In identical experiments in which dilution of rattlesnake venom, a secretion containing much spreading

factor, was substituted for the streptococcus filtrate, but without using India ink, the same inconclusive results were obtained as when the bacterial filtrate was employed.

As regarding the effect of sulfanilamide on the phenomenon, it is difficult to make a conclusive statement in a preliminary note. The individual results will be analyzed separately when the work is fully reported. At present the point with which we are mainly concerned is the phenomenon which occurs with invasive bacteria without the aid of sulfanilamide.

F. DURAN-REYNALS
E. ESTRADA
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INDUCED FORMATION OF β-GENTIOBIOSIDES IN GLADIOLUS CORMS AND TOMATO PLANTS TREATED WITH CHEMICALS

When potato tubers (*Solanum tuberosum* L.) or *Gladiolus* corms are treated with ethylene chlorohydrin in order to break the rest period,[1] the ethylene chlorohydrin absorbed by the tissues is converted into β-2-chloroethyl-*d*-glucoside.[2] Further experiments with other plant tissues and with additional chemicals have shown that the formation of glycosides with the introduced chemicals serving as aglucons can take place quite generally among the higher plants.[3] Unpublished results with carrot roots (*Daucus carota* L. var. *sativa* D.C.) and wheat tops (*Triticum aestivum* L.) have shown that these plants also form β-2-chloroethyl-*d*-glucoside from absorbed ethylene chlorohydrin. However, when gladiolus corms are treated with *o*-chlorophenol, the glycoside formed is not β-*o*-chlorophenol-*d*-glucoside even though corms of the same variety form a β-glucoside when ethylene chlorohydrin is absorbed.[4] The acetyl derivative of this *o*-chlorophenol glycoside from gladiolus corms was prepared by acetylating the material obtained by continuous extraction with ethyl acetate of an aqueous extract of treated corms which had been precipitated with lead acetate, deleaded with hydrogen sulfide and concentrated with reduced pressure. After several recrystallizations from absolute ethanol, it melted at 207.5° to 208.5° (Corr.) and had a specific rotation $[\alpha]_D^{25°} = -49.4°$ ($CHCl_3$, Concn. 2.66 g in 100 cc). Tests with partially purified preparations of the glycoside from aqueous extracts of the corms had shown that on emulsin hydrolysis two moles of reducing sugar, calculated as glucose, are liberated for each mole of *o*-chlorophenol set free, and preliminary studies with the benzimidazole derivatives[5] indicated that both sugars comprising the disaccharide were *d*-glucose. This suggested that the glycoside might be a gentiobioside and accordingly β-*o*-chlorophenol-gentiobioside heptaacetate was synthesized.[6, 7] The melting point and specific rotation of this synthetic gentiobioside were identical with the corresponding values for the isolated acetyl glycoside. Theory for $C_{32}H_{39}O_{18}Cl$: C, 51.44; H, 5.26; Cl, 4.75. Found:[8] C, 51.45; H, 4.97; Cl, 4.60. The propionyl derivatives of both the synthetic and gladiolus glycoside were also prepared and melted at 178.5° to 179° and a mixed melting point determination showed no depression. The glycoside formed in gladiolus corms is thus shown to be β-*o*-chlorophenol-gentiobioside. The quantity of β-*o*-chlorophenol-gentiobioside formed in the treated corms averaged about 0.5 g per 100 cc of expressed juice.

When tomato (*Lycopersicon esculentum* Mill.) plants were grown in sand culture supplied with a complete nutrient solution, and 0.1 to 0.2 millimole of *o*-chlorophenol added daily for about 15 days and then sampled, the roots were found to contain about one millimole of an *o*-chlorophenol glycoside per 100 cc of expressed juice. This glycoside was also β-*o*-chlorophenol-gentiobioside, since the acetyl and propionyl derivatives had the same melting point and showed no depression in mixed melting point determinations with the corresponding synthetic gentiobiosides. A β-glycoside was also formed when tomato plants were grown in the presence of chloral hydrate. The acetyl derivative of the glycoside, melting at 184° to 185°, was obtained from both tops and roots by a procedure similar to that previously used for the preparation of β-2-chloroethyl-*d*-glucoside tetraacetate from gladiolus corms.[2] When trichloroethyl alcohol was added to the nutrient medium instead of choral hydrate, the same glycoside was formed. Synthetic heptaacetyl β-trichloroethyl-gentiobioside, prepared by the reaction between trichloroethyl alcohol and heptaacetyl-bromogentiobiose in the presence of silver carbonate, had the same melting point, and it thus appears that the tomato plant forms β-trichloroethyl-gentiobioside from both chloral and trichloroethyl alcohol. It is of interest that in the detoxication of chloral in the tomato plant, as in animals, a reduction to the corresponding alcohol takes place.

These results indicate that gentiobiose is more widely distributed in plants than was previously supposed.

[1] F. E. Denny, *Am. Jour. Bot.*, 13: 118, 1926; *Contrib. Boyce Thompson Inst.*, 8: 473, 1937.
[2] *Contrib. Boyce Thompson Inst.*, 9: 425, 1938; 10: 139, 1939.
[3] *Am. Jour. Bot.*, 25: 15s, 1938.
[4] *Contrib. Boyce Thompson Inst.*, 11: 25, 1939.
[5] Stanford Moore and Karl Paul Link, *Jour. Biol. Chem.*, 133: 293, 1940. I am indebted to these authors for providing me with a copy of this paper prior to its publication.
[6] The β-octaacetyl gentiobiose used in this synthesis was kindly supplied by Professor William Lloyd Evans, of Ohio State University.
[7] Burckhard Helferich und Ernst Schmitz-Hillebrecht, *Ber. d. Chem. Ges.*, 66: 378, 1933.
[8] Microanalyses by H. Jeanne Thompson.

It is quite likely, of course, that treatments with these chemicals stimulate the production of gentiobiose.

Details of these experiments will be published elsewhere.

LAWRENCE P. MILLER

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CHEMICAL EXAMINATION OF THE LIPID FRACTION OF ROYAL JELLY

HEYL[1] has reported that dilute NaOH extracts or aqueous pyridine extracts of royal jelly, when injected subcutaneously for five days into 21-day-old female rats, caused an increase in follicular activity of the ovaries. Melampy and Stanley[2] have recently failed to confirm this finding. However, it is important to note that the latter workers used *acetone-dried* royal jelly. The use of such a solvent to remove water completely would also remove sterols, phenols, acids, esters, glycerides, etc., some of which are the very types of compounds most likely to possess gonadotropic activity.

For nearly two years the chemical fractionation of royal jelly has been progressing in this laboratory and biological testing of the fractions has been commenced. In a paper at present in press, the authors describe the preliminary results of the chemical examination. Royal jelly contains an ether-soluble fraction of some complexity from which the authors have been able to isolate six substances, and there are indications of several others. Royal jelly, dried to constant weight over P_2O_5 and then powdered, when exhaustively extracted with ether in a Soxhlet apparatus, yields 10 to 15 per cent. of its weight as a cream-colored, semi-crystalline material of waxy consistency. This fraction has a characteristic flowery to spicy odor. The ether-soluble material contains about 0.1 per cent. phosphorus and 0.3 per cent. nitrogen. About 85 per cent. of this fraction can be extracted from ethereal solution by dilute sodium hydroxide. The separation of phenols and acids from this alkali-soluble material, and of wax, phospholipid, sterols and glycerides from the alkali-insoluble portion, has been accomplished. The difficulty of obtaining sufficient quantities of these individual substances delayed commencement of the biological experiments.

However, some feeding experiments performed over a year and a half ago, using the fruit fly, *Drosophila melanogaster,* revealed that the ether-soluble fraction possessed a remarkable influence on the number of eggs laid and the rate of reaching sexual maturity. These results have not been reported because we had hoped to be able to identify the compound responsible for this effect before publishing. The failure of Melampy and Stanley, using acetone-dried royal jelly, to confirm Heyl's results, appears to support our finding that the active material influencing the reproductive system is in the ether-soluble fraction. The detailed results of the Drosophila experiments will be published shortly. Our chemical and biological studies are being continued.

G. F. TOWNSEND
C. C. LUCAS

UNIVERSITY OF TORONTO

SCIENTIFIC APPARATUS AND LABORATORY METHODS

ACCURACY IN ANATOMICAL DRAWING

To encourage greater accuracy in anatomy students' drawings, this year one class was provided with 8 × 10 inch sheets of cellulose acetate (du Pont Plastacele, 1/50 inch thick) ruled in centimeter squares with black auto lacquer. The students themselves prepared 8 × 10 inch heavy bristol board sheets with India ink rulings in centimeter squares (one side), in 1½ cm squares (opposite side), in ½ cm squares (another sheet, one side), and in 2 cm squares (opposite side). Both cellulose acetate and bristol board sheets were numbered vertically and horizontally so that the coordinates of points could be read off with ease.

For drawing, the clear cellulose acetate sheet is laid over the specimen and the appropriate surface of the proper bristol board inserted under the drawing paper. To draw natural size, the surface of the bristol board ruled in centimeter squares goes under the drawing sheet, its rulings clearly showing through the page. To enlarge to 1.5 × the 1½ cm ruling is used, for 2 × the 2 cm ruling, for reductions to 0.5 × the ½ cm ruling. By quickly plotting the chief features of the structures to be drawn on their appropriate coordinates on the drawing paper, the outlines are easily obtained.

Increased facility for making accurate drawings is especially apparent in dissection of nervous or circulatory systems, where simultaneous vision of all related parts is impossible without extensive excision of other organ systems. If the tip of the snout, the sternum or other landmark be fixed upon for repeated reference, the details visible at any one time may be plotted in on the coordinates, then intervening structures moved aside, and the deeper ones exposed. Replacing the cellulose acetate sheet with reference to the chosen point allows the drawing of deeper structures to continue with no distortion. This has proven especially valuable where superficial structures are exposed days before deeper ones. The drawing begun with reference to well-chosen points can be continued after deeper portions are exposed even though the superficial struc-

[1] H. H. Heyl, SCIENCE, 89: 540, 1939.
[2] R. M. Melampy and A. J. Stanley, SCIENCE, 91: 457, 1940.

tures have been destroyed or thoroughly distorted in the interim.

Constant reference of structures in three dimensions to the strictly two-dimensional cellulose acetate sheet makes it much easier to represent these structures than would be believed possible. The drawings show great improvement.

By this method much laboratory time is saved which has hitherto been spent with ruler, measuring these same features of the animal, and in calculating magnifications before drawing the first rough outline. It reduces erasure to a minimum since the squares which show through the drawing paper can be lifted out when not in use. The ruled cellulose acetate sheet in being constantly over the specimen as it is drawn, encourages interest in the accuracy which is so essential in a science laboratory.

LORUS J. MILNE
RANDOLPH-MACON WOMANS COLLEGE

A SIMPLE ARTERY CLIP

WHILE many manufactured artery clips or serrefines are entirely satisfactory, there are none on the market which are inexpensive. This factor becomes important especially in class use or in operations where a great number are needed. An easily made clip was therefore designed which costs only a fraction of a cent.

A glance at the accompanying diagrams will show

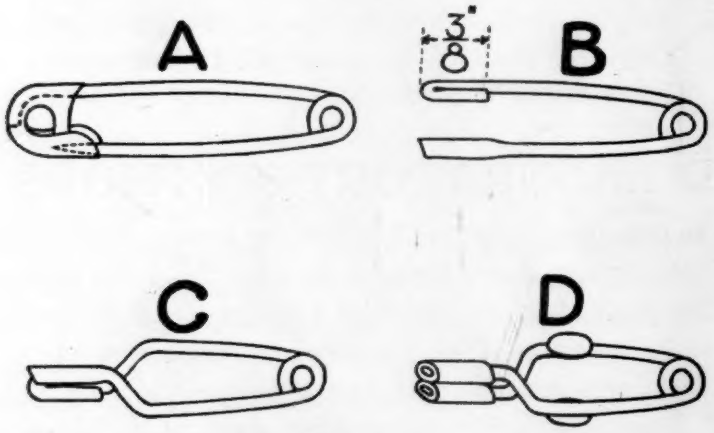

FIG. 1.

the ease with which the clip is made. Sturdy No. 3 safety pins are procured. First, the head is pried off and the head arm is straightened, then bent over (B). The sharp end of the other arm is clipped off and flattened. Next a double bend is put in each arm as shown in (C). The arms may be sprung apart to give the jaws the desired closing power. It is also advisable to bend the head arm counter-clockwise and the sharp arm clockwise to prevent over-riding of the jaws. A quarter-inch copper washer is soldered on each arm and rubber spaghetti tubing is slipped over the jaws completing the clip.

In this laboratory comparison of these clips with ready-made types has not only proved them to be as efficient, but also to be in many cases less damaging to the arterial wall.

J. R. DIPALMA
LONG ISLAND COLLEGE OF MEDICINE

ISOBUTYL METHACRYLATE AS A MOUNTING MEDIUM FOR HISTOLOGICAL PREPARATIONS

RECENTLY O'Brien and Hance[1] advocated isobutyl methacrylate as a substitute for Canada balsam in mounting histological preparations. Since these authors have stressed several advantages of this new plastic over balsam it seems advisable to point out that this substitute has proved unsatisfactory with certain histological techniques used in this laboratory.

Celloidin sections, measuring about 30×50 mm and 25 micra thick, through the entire hemisphere of a macaque's brain were stained with thionine for nerve cells (Nissl) or according to the Weil method for myelinated nerve fibers. Such sections mounted without a cover glass (as were the preparations of O'Brien and Hance) in xylene solutions of various concentrations of isobutyl methacrylate polymer (du Pont), warped on hardening and the surface of the medium roughened. Mounting such sections in this medium under a cover glass caused cracks and fissures in the preparations. Moreover, it was found that whereas the stain in the Nissl preparations was unaffected, the bright blue of the myelin sheaths in the Weil preparations turned to a dull gray in this medium and some of the finer detail was lost.

JOHN MEACHAM HAMILTON
YALE UNIVERSITY SCHOOL OF MEDICINE

[1] Harold C. O'Brien and Robert T. Hance, SCIENCE, 91: 412, 1940.

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COHEN, JUNE B. and MILTON B. COHEN. *Your Allergy . . . and What to Do About It.* Pp. 177. Illustrated. Lippincott. $1.50.

FEIGL, F. *Specific and Special Reactions; For Use in Qualitative Analysis, with Particular Reference to Spot Test Analysis.* Translated from the German by RALPH E. OESPER. Pp. ix + 192. Nordemann. $3.50.

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